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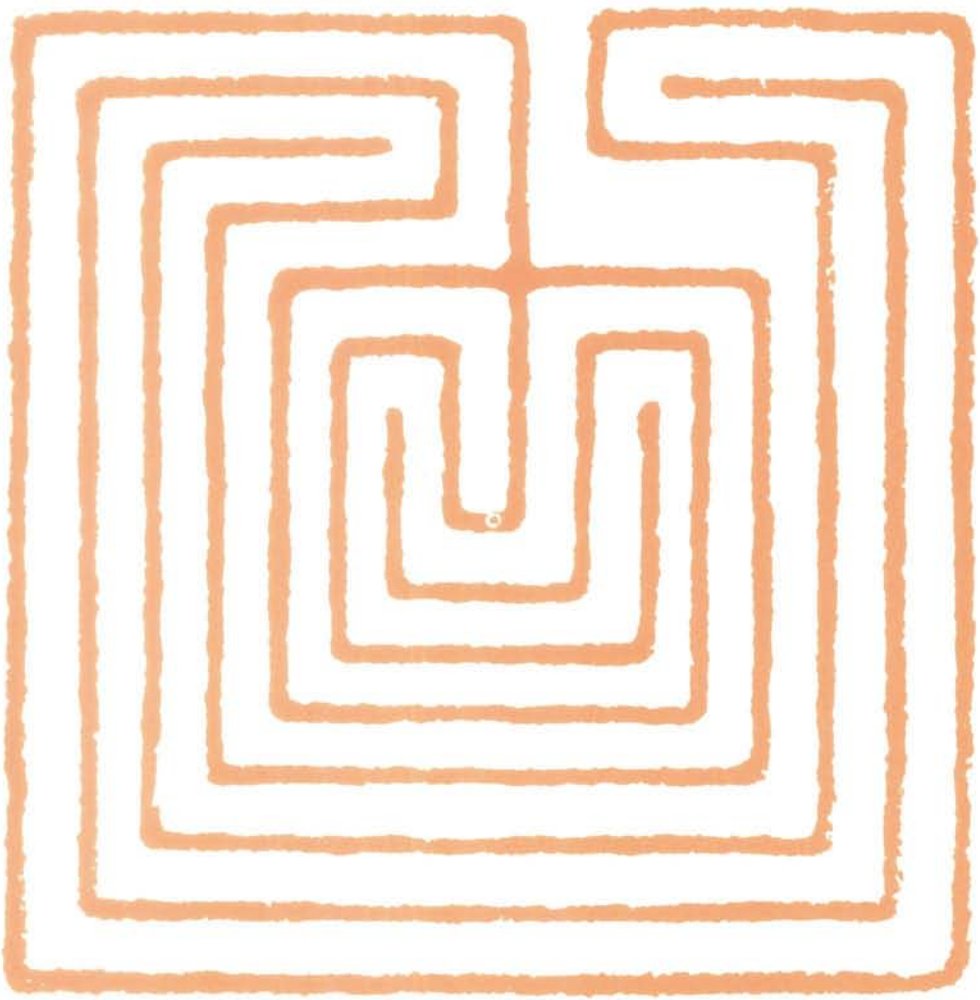
Discoveries and Interpretations
Studies in Contemporary Scholarship

Volume I



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Discoveries and Interpretations

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Journal of the American Academy of Arts and Sciences

Discoveries and Interpretations
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SUMMER 1977: DISCOVERIES AND INTERPRETATIONS: STUDIES IN CONTEMPORARY SCHOLARSHIP, VOLUME I

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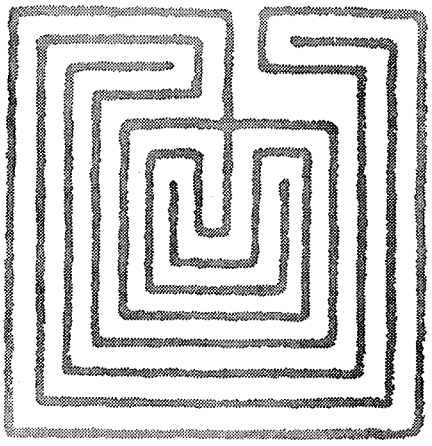

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STEPHEN R. GRAUBARD

Introduction

WHO, IN THE LATTER PART of the twentieth century, would choose to risk his reputation by claiming that there has been no time more propitious for the advancement of learning than our own? Who, in short, would wish to praise this age in the terms that Francis Bacon used, quite deliberately, in extolling his own? Bacon knew only too well the inadequacies of the "scholarship" of his day; this did not prevent him from saying, in 1605, that the prospects for learning were incomparable.[1] Have the events of the twentieth century foreclosed all possibilities of such expressions of pride and self-confidence? Or, was it not pride at all that led Bacon to say these things about learning in the seventeenth century? Why, then, are such statements almost inconceivable in our time? Are they in fact inconceivable?

These questions are not asked rhetorically. They derive very naturally from the essays published in this issue of *Daedalus*, and also from those planned for the issue that is to follow. Each volume is concerned with contemporary scholarship; each traverses many fields, suggesting how traditional disciplinary boundaries have shifted, and why research priorities today are substantially different from what they were even a few decades ago. In certain areas, the "progress" in scholarship is undeniably impressive; in others, where substantial difficulties are reported, the situation is more ambiguous. In no instance, however, is the situation said to be bleak. When, for example, Dr. Lewis Thomas, intentionally provocative, insists that "we are *dumb*,"[2] his purpose is only to remind us that medical research may be approaching a time of such basic discovery that all earlier biological findings will seem almost inconsequential by comparison. Nature is still largely unknown, Dr. Thomas implies; it waits to be explored, and ought to be.

Lest this be thought the conventional optimism of the scientist—quite different from the habitual despondency of the contemporary humanist, on which Walter Jackson Bate has written so eloquently[3]—it must be said that this is not a distinction that figures in these volumes. Those who write on humanistic scholarship are as hopeful for its development as those who write on the natural sciences. Indeed, if there be any area where significant misgivings are expressed, and where a certain disappointment is registered, it is in the social sciences. Whether the conditions for scholarly advance are in fact less auspicious there, or whether it is simply that those who write from within the social sciences are more critical—made so by their professional formation—is a matter for readers to decide.

In any case, these volumes are not testaments of gloom. On the contrary; they report and interpret significant scholarly innovations in many disciplines and areas. They also provide an incomparable insight into certain of the in-

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tellectual presuppositions of our time. Why then, for all the reports of "progress" in individual fields, is there not more here that is redolent of Bacon's *Advancement of Learning*? Why is individual achievement constantly remarked on, with new analytical methods and technologies singled out for praise, without there being a corresponding enthusiasm for the new social and political conditions that make for creativity? Does this suggest a certain lack of confidence in the institutions that sustain scholarship today? Or, is some other explanation more likely?

The individual authors, asked to estimate the character and quality of research in particular areas, were effectively precluded from seeing scholarship in the large; the reader of these *Daedalus* issues is not similarly disadvantaged. Instructed by what is contained in these pages, and able to supplement this with other information, he may come to see the contributions of contemporary scholars in a quite new perspective. To a greater or lesser extent, all scholars today live within disciplinary enclaves; they know best those languages, symbol systems, and modes of argument common to themselves and their professional peers. Because none of these is universally shared—but for other reasons as well—the full dimensions of contemporary research are little known.

Yet, scholarship has been fundamentally transformed in this century; the rapidity of certain developments—their complexity and variety—justify close and critical scrutiny. Not only within individual professions and disciplines have conditions changed, but few institutions committed to the advancement of learning pursue their purposes today in the way they once did. Systems of patronage are greatly altered; the growth of public funding, both in the United States and elsewhere, has served to alter priorities in research. This, together with the relative decline of Europe as a center of world learning, an event only partially eclipsed by the growth of American influence, makes the advancement of learning a very different thing today from what it was a generation or longer ago.

All this has happened in the context of a growing doubt about the utility of certain kinds of learning, and an increasingly acrimonious debate in many parts of the world about the adequacy of established educational institutions and practices. At one level, these new complaints only repeat what scholars (and others) have been saying periodically since the latter part of the eighteenth century. Still, the skepticism today seems particularly acute; misgivings are more openly proclaimed; countervailing expressions of confidence are less common. These facts need to be recalled as one reads of major "advances" in individual disciplines. Each of these volumes invites a response that only the individual reader can provide from the perspective of his own discipline or profession. What follows, then, is a set of not entirely random observations by someone who reads these issues with the lens of the historian, who finds acceptable the idea that certain times are more congenial to learning than others, and who asks how our own times are to be judged.

The question implies a willingness to compare this century with others; the seventeenth and the eighteenth centuries offer themselves as likely candidates. How can one explain the extraordinary creativity of these earlier ages? What led Francis Bacon, for example, to contend with powerful adversaries, believing that they despised learning and felt threatened by it, but knowing that they

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could not possibly hold out against those, like himself, who were determined to terminate the rule of scholastic philosophy and Aristotelian physics? How does one account for his conviction that an age no less distinguished than that of ancient Greece and Rome was impending? On this last point, Bacon was explicit; he wrote of the "excellency and vivacity of the wits of this age," but also of "the noble helps and lights which we have by the travails of ancient writers; the art of printing, which communicateth books to men of all fortunes; the openness of the world by navigation, which hath disclosed multitudes of experiments, and a mass of natural history; the leisure wherewith these times abound . . ."[4] He wrote also of the virtues of peace and of the advantages secured through the end of fratricidal religious wars. These, obviously, helped create conditions propitious for learning, as did the patronage of the king, whom he was not unwilling to flatter.

Bacon offered explanations that others found wholly credible; when, in 1948, Herbert Butterfield suggested that the "scientific revolution," broadly defined, "outshines everything since the rise of Christianity and reduces the Renaissance and Reformation to the rank of mere episodes, mere internal displacements within the system of medieval Christendom,"[5] no one charged him with hyperbole. Bacon had laid the basis for such a judgment; indeed, he had gone further. Describing what the new learning would entail in the way of institutional arrangements, he had written: "I take it that all those things are to be held possible and performable, which may be done by some persons, though not by everyone; and which may be done in the succession of ages, though not in one man's life; and lastly, which may be done by public designation and expense, though not by private means and endeavor."[6] The new scholarship would, in short, be professional, cumulative, and funded by public monies.

If the scholarship that developed from these new procedures rapidly invalidated the theories of certain ancient authors, particularly in the area of the natural sciences where wholly new possibilities of observation and verification existed, this did not result in the abandonment (or neglect) of major classical texts. On the contrary; the new learning, precisely because of its overwhelming concern with textual accuracy, gave a powerful incentive for many new kinds of humanistic inquiry. The seventeenth and eighteenth centuries were as distinguished for their humanistic as for their scientific scholarship. When Isaiah Berlin, in his recent work on Vico and Herder, reminds us of the importance of the new historicism—in part an outgrowth of this growing textual preoccupation—he indicates that it was not history alone that was influenced, but philology, jurisprudence, anthropology, and a host of other subjects having to do with the character of civilization.[7]

The respect for the new learning was immense; in Europe, particularly in France, the most exaggerated claims were made for it. Frank Manuel, in writing on "the French idea of perfectibility," suggests that Voltaire and Turgot believed "that the values of contemporary French scientific and rationalist society would endure forever, growing in strength throughout all time: the modern Antonines would never decline."[8] Condorcet went even further; he expected man to learn all of nature's laws, eventually taming nature, and becoming in time a power equal to nature. Manuel calls this "one of the grandest expressions of hubris in an age not characterized by excessive humility."[9]

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Not everyone, however, was so accepting of the new learning. Rousseau, perhaps more than any other in the eighteenth century, questioned its value and wondered whether it was not debasing society. Rousseau had little use for the "high culture" of his day, believing that it only perpetuated inequalities that he found offensive. Whereas others gloried in the literary, artistic, and scientific achievements of the Enlightenment, he saw them as corrosive, mocking the virtue of ordinary people. Rousseau believed, and others agreed with him, that to be alive in the eighteenth century was to be witness to a time of moral declension.

Although it would be difficult to explain the appeal that Rousseau's work had in Germany, Frank Manuel scarcely exaggerates when he says: "The two Discourses [by Rousseau] were commonly read in Germany as a history of man's moral fall from the state of nature, a decline that inevitably accompanied the progress of the arts and sciences, the growth of technology, the increase of competitive pride, the accumulation of instruments of domination, and the spread of luxury, softness, and effeminacy."[10]

By the end of the eighteenth century and the beginning of the nineteenth, talk of the decline of *Bildung*, education and its relation to moral character, was common in Germany. Hegel was only one of many who wondered whether modern society was not becoming despiritualized, losing all social cohesion, no longer providing models for effective self-development. Judith Shklar, in her recent study of Hegel's *Phenomenology of Mind*, writes of a society where "the division of labor, the enormous distances between classes, the geographic and political particularism, the isolation of the educated from the rest of the population and a privatizing piety, all seemed to contribute to a non-society held together only by mechanical coercion from the top."[11]

To read Friedrich Schiller is to be made aware of how deep was the anxiety, and how concerned he (and others) were to remedy conditions that they thought threatened the integrity of their society. Schiller recommended a drastic overhaul of education; only through art, he argued, could the individual be protected in his dignity and restored to some meaningful relation with his fellows. Schiller wrote, and not without asperity, of the frontiers of art being restricted even as those of science were enlarged; he said: "*Utility* is the great idol of the age, to which all powers must do service and all talents swear allegiance. In these clumsy scales the spiritual service of Art has no weight; deprived of all encouragement, she flees from the noisy mart of our century."[12]

If certain of these late eighteenth- and early nineteenth-century reflections seem familiar, it is not because today's critics, aware of the historical record, have returned to it, repeating old arguments in a new idiom. Rather, it is that the wheel does risk being constantly reinvented. Many aspects of the contemporary crisis in learning were already obvious to intellectuals in the eighteenth century; the difference, however, is that those who interpreted the situation in the way Rousseau and Schiller did were members of a distinct minority almost everywhere; in any case, their views did not carry the day, least of all in the United Kingdom, where comparable quotations can be culled from the writings of many, including the Romantics. The overwhelming European sentiment, however, clearly favored the vaunted industrial progress that was occurring; pleasure and pride were felt for many of the highly advertised intellectual and cultural accomplishments of the day, which were by no means inconsiderable.

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To insist on these points is simply to emphasize what may otherwise be forgotten; misgivings about science and scientific method are in no sense novel. The opposition to both came not only from those who were ignorant—of whom there were many—but also from intellectuals, poets, and scholars who refused to acknowledge what seemed so obvious to the majority, that the new learning carried the prospect of new insight and new power. The skeptics looked principally at the costs, which they refused to reckon in pounds and pence, and were not enthusiastic about what they found. A debate was initiated in the eighteenth century that has continued from that day to this.

Although no one in the eighteenth century thought to argue that "the advancement of learning" was owing largely to the exertions of relatively small numbers of people, concentrated in a handful of European cities, and heavily dependent on various privileged systems of patronage, such was in fact the situation. The intellectual achievements of the century were overwhelmingly European; again, a point not frequently adverted to. When, therefore, a man reputed for his wit and learning, and not in the habit of venting his spleen, found it necessary to disparage the intellectual and cultural achievements of another society, the American, that fact was noted. The attack came from Sydney Smith, one of the founders of the *Edinburgh Review*; in 1820, he wrote:

> In the four quarters of the globe, who reads an American book? or goes to an American play? or looks at an American picture or statue? What does the world yet owe to American physicians or surgeons? What new substances have their chemists discovered? or what old ones have they analyzed? What new constellations have been discovered by the telescopes of Americans? What have they done in the mathematics? Who drinks out of American glasses? or eats from American plates? or wears American coats or gowns? or sleeps in American blankets? Finally, under which of the old tyrannical governments of Europe is every sixth man a slave, whom his fellow creatures may buy and sell and torture?[13]

Each of the criticisms—but the last sentence particularly—expressed exasperation with a people known to be boastful and given to exaggerating their qualities, who clearly needed to be put in their place. Smith did so in a way that could not fail to be hurtful to American pride. Rather than find fault with the American democracy, with its experiment in "popular government" that offended certain other of his compatriots, he chose to remind Americans that their intellectual achievements were nil, and that the world owed them little in those domains where civilized peoples generally tried to excel. The barb was meant to wound; it did. Try as Americans would to romanticize their accomplishments in the sciences, or make their constitutional experiment seem a worthy substitute for what Europe was accomplishing in both the arts and the sciences, Smith had detected a weak point in the American armor. He might have discovered the same vulnerability in any number of other countries; he did not look for it there.

All this is significant principally because it reminds us of the extent to which the world's learning, even as late as the early nineteenth century, depended on creative intelligence overwhelmingly concentrated in a small corner of Europe. The United States, insofar as it had an indigenous culture or intellectual life of its own, was thought to have borrowed it largely from Europe, principally from Great Britain and France. The same could be said of Russia, the other great

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continental state that owed much of its intellectual accomplishment to selective borrowing from others, particularly Germany and France. If the United States seemed a European "outpost" beyond the Atlantic, Russia was Europe's "outpost" in the East. Neither the United States nor Russia could boast of having produced learned men comparable to Newton, Descartes, Hegel, Locke, Bayle, Kant, Hume, Montesquieu, Spinoza, or any number of others who made up a pantheon of modern "greats," equal to the best that had come out of Greece and Rome.

When, in 1835, Alexis de Tocqueville prophesied the emergence of these two states, the United States and Russia, that had "grown in obscurity," but whom he expected "by some secret design of Providence one day to hold in its hands the destinies of half the world,"[14] he said almost nothing about the United States as a center of learning; neither its colleges, libraries, nor museums justified his attention. Nor did he have great hopes for American science, of which he said: "Nothing is more necessary to the culture of the higher sciences, or the more elevated departments of science than meditation; and nothing is less suited to meditation than the structure of democratic society."[15] The uniqueness of America, for Tocqueville, lay not in its learning, but in its passion—its passion for equality.

Learning, as we normally define that term—its advancement, certainly—remained something of a European monopoly for the greater part of the nineteenth century. Individuals in the United States and elsewhere made specific contributions scarcely less important than those made in Europe, but nowhere outside Europe was there that "critical mass" of talented and trained investigators who could compare with those in Europe.

That situation began to change in the last decades of the nineteenth century; although it would be a mistake to ascribe the change to any single institutional development, or to suggest that it was precipitous, it was first apparent in the United States, and was considerably more obvious after the Civil War than before; it was linked to the creation of many new institutions, not the least important, perhaps, being universities.

In Europe, the new learning had advanced despite the weakness of the universities. Indeed, in both England and France, in the seventeenth and eighteenth centuries, and for some part of the nineteenth, the new scholarship seemed largely emancipated from university inspiration, control, or administration. When Americans came back praising European universities, and they began to do so early in the nineteenth century, it was generally the German universities that they praised, which owed much to the reforms of Wilhelm von Humboldt. When they went abroad in search of knowledge, and, later, to search for scientific equipment, Americans went as often to Germany as to either France or England.

Yet, in 1862, when Congress passed the Morrill Land Grant College Act, which appropriated public land to states willing to establish agricultural and industrial colleges, the inspiration for the legislation was wholly American. While Europeans might look askance at these narrowly utilitarian institutions, insofar as they looked at them at all, Americans assumed that they had the same right to determine what a university should be as those who might claim to be upholding traditions dating back to the eleventh, twelfth, or thirteenth centuries.

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It is always difficult to trace the sources of self-confidence; indeed, it may be a fruitless search. We know, however, that specialization in science, together with a growing professionalization, became increasingly common in the United States in the 1830s and 1840s; more than that, and despite opposition, the federal government took an increasingly active and positive role in the support of science.[16] Even more important, perhaps, in providing the basis for American self-confidence was their success with technology. A capacity for innovation and invention and for using the discoveries of others, for knowing how to convert these to American needs while making major discoveries of their own, seemed a well-established American trait even before the Civil War. In fitting steam engines to ships, for example, the Americans made one fleet of vessels obsolete, but proceeded quickly to build another; their skills made a new level of transatlantic trade possible. On land, the story was the same; the invention of the McCormick reaper vastly increased the possibilities of grain production. Rail and ship were used to convey this vast new yield to Europe, bringing in return armies of impoverished immigrants, ready to do the heavy work that a newly industrialized society required.[17]

It is not enough, however, to point to single inventions—the Singer sewing machine, Bell's telephone, Edison's electric light bulbs—to represent the progress that the United States made in technology during these years. Daniel Boorstin captures the spirit of the post-Civil War decades when he chooses to write of "The Go-Getters"; he says:

> The years after the Civil War when the continent was only partly explored were the halcyon days of the Go-Getters. They went in search of what others had never imagined was there to get. The Go-Getters made something out of nothing, they brought meat out of the desert, found oil in the rocks, and brought light to millions. They discovered new resources, and where there seemed none to be discovered, they invented new ways of profiting from others who were trying to invent and to discover.[18]

That all this enterprise and energy created venality and vulgarity, Boorstin recognizes. Henry Adams was only the most articulate of his generation to cry out against what was happening, and to regret the passing of a more simple and virtuous America. But it was not only the dynamo that triumphed in these frenetic years, nor was it only useful knowledge that came to enjoy respect and patronage. Slowly, but surely, America transformed its colleges—until then essentially schools for adolescent boys—into institutions having a wider set of social purposes. Old establishments were expanded, new ones were founded, on both coasts but also in the center of the country.

Although some wrote as if the United States had simply imported a set of German ideas on higher education, grafting these onto a stock that remained essentially Anglo-American, the operation was considerably more complex. In only the most superficial ways did the American universities come to resemble Heidelberg, Berlin, or Göttingen; as for their resemblances to Oxford and Cambridge, these existed mostly in the minds of those who knew neither. A "seminar" did not a German institution make, nor did a "common room" bring British academic ways to America.

What did happen, however, was the reinforcing of a commitment to scholarship, learning, and research—negligible in many American colleges up to that time—but suddenly a claimant to influence, in competition, obviously, with

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other interests. Since this development coincided with many others—not the least important being the rich man's mania for collecting books, paintings, sculpture, manuscripts, and other objects of value—libraries, archives, and museums proliferated in a way they had not done in the first century after the country's independence. The United States had long thought of itself as a naturalist's paradise; with its new impressive collections of books and artifacts, it became a haven also for scholars in many scholarly disciplines.

Whether the explanation for this explosion of intellectual interest and of institutional innovation is looked for in the new economic prosperity of the country, particularly among its industrial leaders, hoping to immortalize themselves and their families through philanthropy or wanting to give one final testimonial to their religious faith, or whether it is looked for in the rise of a new generation of intellectual entrepreneurs, as capable (and no less ambitious) than their friends, the "captains of industry," there can be no denying the fact that the United States, during these tumultuous decades, assumed a new position of importance as a center of learning.

One must be careful not to exaggerate the extent of the change, or to attribute to 1890 or 1910 what took place only in 1930 or 1950, but the founding of new professional associations, the creation of outstanding archives, and the building of major research laboratories gave the United States an international intellectual standing it had not previously enjoyed. It is no accident that the reform of professional instruction in both law and medicine also took place at this time. That the Carnegie Institution of Washington and the Rockefeller Institute for Medical Research—both established to do "pure" scientific research—were founded in the first decade of the twentieth century, along with the Bell Telephone Laboratories and the Research Laboratory of the General Electric Company, both committed to furthering industrial research, tells something of the range of America's new research ambitions.

If the growing importance of American scholarship is emphasized, this ought not to be confused with an assertion of American superiority or supremacy. Scholars in the United States did not attain the kind of eminence in the late nineteenth or early twentieth centuries that made very many of them the intellectual rivals of their European peers. Individuals will have to judge for specific disciplines how great the ascent was. When, however, one considers a discipline like economics, where many would say that the United States is now eminent (some would suggest, pre-eminent), the nineteenth-century record appears slight. In a century which produced Malthus, Ricardo, Mill, Jevons, and Marshall in England—to name only the most distinguished—the United States could boast of Francis Walker, Henry George, John Bates Clark, and Thorstein Veblen. The situation in economics resembled that of most other disciplines, in the natural sciences and the humanities no less than in the social sciences.

In any history of scholarship in the twentieth century, the years 1914–1918 have a very special importance. Thorstein Veblen, the maverick American economist, was one of the first to recognize this. In his 1918 edition of *The Higher Education in America*, he raised a question that had not previously been much attended to; he asked how the war was likely to affect America's standing

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in the world of scholarship. What might the "insolvency of the European academic community" do to raise the prestige of the American? Veblen recognized the importance of the high European casualty rates, but worried also about the moral losses that were being incurred. The "current and prospective loss of morale" concerned him; so, also, did the "serious diversion of the young forces from learning to transiently urgent matters of a more material and more ephemeral nature." The Americans, he knew, had reason to expect to emerge from the war with their "holdings in the domain of science and scholarship . . . in the main, still intact."[19] Veblen asked what America would do to help in the intellectual reconstruction of Europe. He had no doubt that such a reconstruction was essential.

That the United States aided individual European institutions after 1918 is well known; that it never developed a policy for European "intellectual reconstruction" ought also to be acknowledged. The idea that such a "reconstruction of learning" was in America's interest—that indeed it could be effected—was never seriously considered. Few Americans stopped to ask how the "advancement of learning" in the twentieth century might be influenced by Europe's tragedy, not least by the destruction of a generation of potentially able scholars.

The accomplishments of American scholarship in the 1920s and early 1930s have been insufficiently reflected on; the activities of certain American foundations, the Rockefeller Foundation, most importantly, in fostering the exchange of scholars, and in bringing large numbers of European scientists to the United States (years before the advent of Hitler) is all too rarely mentioned. So, also, too little attention has been given to the reverse flow; in the 1920s European scholarship was still impressive; many senior professors, too old for active military service, survived the war; Americans knew of their reputations and flocked to study under them. One does nothing to diminish the importance of the "intellectual diaspora" caused by Hitler's racial policies—one may indeed give it a new dimension—if one recalls that the American willingness to receive threatened European scholars reflected in part an appreciation of what Europe represented in the world of learning; this knowledge was based in part on exchange visits begun before the war, but increasingly common after 1918.

No one living in 1914, concerned with the advancement of learning, could have predicted the catastrophes that overwhelmed Europe in the decades that followed. In 1936, when Harvard celebrated its tercentenary, Germany no longer figured as a major center of scholarship. A sufficient number of its more distinguished professors, compelled to leave or choosing to leave, abandoned a country riven by ideological and political turmoil. In Russia, under Stalin, the situation was not appreciably better. Nor, for that matter, could it be said to be very satisfactory in Italy, where fascism imposed its own restraints and created its own exiles. France, still suffering from the effects of the indescribable slaughter of World War I, had never regained the scholarly renown it had enjoyed before 1914. As for the United Kingdom, its "gains" and "losses" were more difficult to establish. Did certain of its scholarly accomplishments appear more considerable because of the flatness of the surrounding terrain—particularly because of the decline of Germany—or were they in fact fairly substantial? The same question, obviously, could be asked of the United States, but by the late

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1930s and early 1940s, that question admitted of only a single answer. Learning in the United States was advancing; Europe's tragedies were serving to change the world intellectual scene.

What began as a trickle, early in the 1930s, became a flood by the end of the decade. Some of the most accomplished men and women of Europe abandoned their homelands; where some sought temporary (or permanent) refuge in the United Kingdom, the greater number made their way to the New World, principally though not exclusively to the United States. In a depression-ridden economy, the absorption of large numbers of scholars proved something of an embarrassment. Posts were rarly available; they had to be created, often at salaries substantially below those enjoyed by others of less distinction. Many foreign scholars were shunted off to small, inconspicuous institutions. The transition to the United States was rarely easy, even for those who came with established reputations. However, no one could doubt that a major resource had been added to the American scholarly community, particularly to its colleges and universities.

We have only barely begun to explore the changes produced by this incomparable infusion of talent.[20] The refugee contributions to the natural sciences—and not only in the construction of the atom bomb—is a fragment of a much larger story that involves major contributions to the arts, the social sciences, and the humanities. Although the full account of the scientific contribution—involving the creation of a vast new bureaucratic apparatus, not only at Oak Ridge, but through the Office of Scientific Research and Development, the National Defense Research Committee, and the Committee for Medical Research in other places as well—is only now beginning to emerge, the prestige of science soared as a consequence of what happened during the Second World War. In 1940, the total national research and development expenditure was approximately $74 million; a decade later, at the beginning of the Korean War, it stood at $1.2 billion; by 1959, after the excitement generated by Sputnik, it had reached $5.8 billion; at the time of President Kennedy's death it was $11.9 billion.

Although it would be a mistake to suggest that this vast infusion of public funds skewed all scholarly activity, it could not fail to alter substantially the character of American scholarship. Agencies of the United States government emerged as major patrons, prepared to pay for the kinds of research they deemed useful. Even though peer review systems guaranteed that certain standards were maintained in the granting of funds, such review could not divert funds from one range of issues to another. Also, as the federal government became more calculating in its appropriations, showing a clear preference for one sort of research over another, individuals and institutions accommodated themselves to federal needs. Vast new armies of researchers appeared, not only in universities, but in a whole range of independent para-academic institutions that sometimes enjoyed nominal associations with universities. A few universities became, in effect, federal-grant institutions, though all would have resisted strenuously the implications of such a characterization. Entire disciplines—medical research, for example—accepted the fact of their federal sponsorship.

In an earlier time, in Europe, military needs had provided the stimulus for both theoretical and applied scientific study. Why should it not do the same in

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America, particularly if certain freedoms were guaranteed researchers? Besides, if military necessity legitimated certain forms of scholarship, who could argue against scholarship committed to the service of health, the city, or the environment? If money was "clean" when it came from the hands of industrialists or philanthropists, private corporations or foundations, why should it be suspect when it came from civil servants, disbursing public funds voted by Congress for public purposes? The fact that the money came in unprecedented amounts, more frequently for certain purposes than others, seemed scarcely important.

Everything in America seemed to be expanding. There appeared to be no limits to what America's scholarly institutions might not be able to do. In the early 1960s, no one spoke of the vulgarity of this restless intellectual adventure. Why should it be thought vulgar? While there was a certain amount of self-seeking, of course, and "boondoggling" was not unknown, the greater part of the expenditure seemed related to genuine public purposes, responding to specific public needs. The universities, together with all their creations, prospered as they had never previously done.

American scholars attained a new eminence. In many disciplines, particularly outside the natural sciences, Soviet Russia's contributions seemed only marginal by comparison. Great Britain, once so influential, seemed somewhat reduced in stature, though individual contributions in specific fields continued to draw attention. So, also, with France. As for Germany, while its recovery from the trauma of Hitler was rapid in many respects, this was not as obvious in the intellectual as in certain other spheres. Germany was no longer the center of major research in any number of disciplines where it had clearly dominated through the 1920s. In the applied sciences—in space research, weapons development, computers, and medical technology—but also in the pure sciences, the American achievement seemed unprecedented. Social science, for a time, appeared to be almost a special reserve marked out for American exploitation. Even in the more traditional humanistic disciplines, where Europe had long enjoyed supremacy, the United States seemed suddenly pre-eminent. Or so, at least, many in the United States insisted; there were few international prizes in these latter disciplines to judge the validity of the claims made. But bibliographies told part of the story, as did other indices, which showed a growing worldwide citation of American scholarly books and papers. These conditions did not induce modesty, either in the disciplines that were burgeoning and growing, or in those that remained more tranquil, absorbing something of the excitement generated by the others.

Americans, in the late 1950s and early 1960s, were not disinclined to boast of their scholarly achievements. In Tocqueville's day, American immodesty had touched other things; in the early 1960s, to a degree that is now sometimes forgotten, it touched—and not always lightly—scientific and scholarly eminence. The country's aptitude for spectacular invention had in no way diminished; the explorations in space and the flight to the moon gave evidence of that. For those who valued more theoretical scientific capabilities, America's record seemed no less spectacular.

It is difficult to know when the first shocks to this excessive pride were recorded. Some will argue for Berkeley, in 1965, where the dubiety of students about the exaggerated claims of professors were first registered. Others will see

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the development of criticism in more global terms. The war in Vietnam, they will argue, was a major reason for the marked revulsion against universities, principal beneficiaries of federal patronage. Institutions that had not previously been subjected to persistent public scrutiny—at least not to hostile scrutiny—discovered that claiming a worthy purpose was no longer a sufficient defense. A larger vindication of past conduct was demanded. Some were frightened that certain of these institutions might not be able to survive the attacks; others, more sanguine, imagined that the tumult and the disorder might in the end have a cleansing effect. In time, the tumult subsided; the end of the Vietnam War, a growing preoccupation with Watergate, and economic recession diverted attention from all such issues. By 1977, most colleges and universities were struggling principally to make their budgets balance; other issues were set aside for another time.

These are the conditions that obtain in the United States at the present time. "Normalcy" has taken hold, and America's institutions of higher learning appear as they have long been—independent, diverse, short of funds. The federal patronage, although it does not grow in the way that it once did, continues at very high levels. If the hubris of the late 1950s and early 1960s is somewhat attenuated—and almost all would agree that it is—this does not imply disenchantment within the system or despair about it. Creativity, by almost every criterion, is still common; the "advancement of learning" remains a prime goal. There are, in short, powerful incentives for individuals and institutions to seek to advance their fortunes in the old way, taking advantage of the opportunities that present themselves. Specialization remains all-important, and the *Daedalus* essays suggest why this condition cannot be expected to change. The benefits from the prevailing system are obvious. What, however, of the losses?

In 1972, in his Jefferson lecture, Lionel Trilling spoke of these; he began by recalling the opinions of the third President: "He [Jefferson] held the view, which was characteristic of the eighteenth century, that men were essentially equal in their mental faculties. This is not to say that all men have the same speed, or agility, or strength of mind, only that all men have reason and that the intellectual resources of a nation are invested not in a few but held in common."[21] Who would be willing to espouse such a philosophy today? Trilling spoke of the scientific illiteracy of the twentieth century, of the great numbers excluded from the possibility of knowing or understanding anything of contemporary science. The situation was not very different in the humanities or in the social sciences, Trilling explained. The whole Jeffersonian expectation, that the educated man would have access to science and mathematics, to literature and history, and that all this would be comprehensible to him, lay in ruins. Trilling, like others, believed that what is really serious in our day is not only that physicists "speak" only to physicists (and not always to all of them) or that economists cannot write as Malthus, Mill, or Keynes did—thereby effectively barring the general public from their works—but that this "exclusion of most of us from the mode of thought which is habitually said to be the characteristic achievement of the modern age is bound to be experienced as a wound given to our intellectual self-esteem."[22]

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Trilling might have gone further to ask whether the uneasiness of many about the conditions that exist today does not derive from the fact that many of the most serious problems of our time are written about principally by journalists and others who have neither the time nor the inclination to study these matters closely. One may argue about whether it is crucial for monographs, written in a specialized idiom, to be "translated" for a lay audience; some will insist that the future excellence of research in physics, mathematics, art, history, geology, sociology, and anthropology does not depend on communication with an educated lay public. No one will suggest, however, that there is no loss when the public is denied access to serious reflection on substantive issues that touch their lives intimately.

International exchange on a scholarly level proceeds as effectively today as it has ever done; the American contribution to such international communication is unprecedented. Incomparable facilities exist for the publication of scholarly materials. What is missing, today, missing for the American citizen, but missing also for others curious to know what is being thought, are the nontechnical but serious reflections of men and women able to dwell on large public issues, acknowledging their complexity, but striving for the same precision in these matters that they would seek to attain in writing for their professional peers. To put the matter bluntly, there is too little support today for the research of the generalist. The federal government, patron-in-chief of much that is most commendable in monographic research, rarely supports such general inquiry. Some would say that it is no longer a genre that has much appeal for scholars, committed to other kinds of enterprise.

There is no avoiding the issue: professional organization, university curricula, public and private patronage, considerations of prestige and power—all these conduce to make men and women of ability seek to prove their "expertness" through their acceptability to their professional peers. Originality is judged by what peers say of an individual's work, and there is no reason to decry this system. When the individual requires time to complete a scholarly project, facilities are generally made available. Where the experiment, project, or book is deemed valuable, the individual (or team) will generally be provided with the wherewithal to complete successfully what he or they have initiated. Not the least of the achievements of American twentieth-century scholarship is the creation of incomparable facilities for research. No other nation can claim equivalent libraries, laboratories, research centers, and archives. These are remarkable in every respect, and no words of praise can do full justice to their qualities. All, however, are organized principally to serve specific disciplines, particular interests; they exist to make "monographic scholarship" possible. This is not to say that they could not be used also for other purposes; they are not being so used today.

Who, today, would write as Bacon did in 1605, arguing that the most propitious time for learning is at hand? What evidences would such a writer adduce to support his opinion? Where would he look for the modern equivalent to printing? To the computer? To videotape? Where would he find the contemporary analogue to the new terrestrial worlds that the men of the sixteenth and

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seventeenth centuries discovered? On the moon, in the oceans, in other parts of the planetary system? Where would he search for the new patronage? To whom would he address his pleas for help? To the bureaucratic state, increasingly required, at least in the liberal democratic West, to arbitrate and negotiate between an ever larger number of claimants? Or, are the new states of Africa and Asia the hope of the future, for the advancement of learning as for so much else? Can new methods, new approaches—the construction of wholly new archives and new systems of classification—together with an increased tolerance for new kinds of questions, provide the excitement that will make the last decades of the twentieth century an extraordinary time for learning? And if so, extraordinary for whom?

Or, are all such hopes chimerical? Must the purpose not be much more ambitious? Is it possible that while many individual disciplines thrive, even as these volumes suggest, the larger learning enterprise is faltering? May the greatest need of our time not be the rehabilitation of learning, not only to contradict those who disparage reason and peddle newfangled mysteries, but to serve populations who now perceive dimly, if at all, what the human intelligence aspires to create?

Contemporary scholars, particularly in the liberal democratic West, would do well to heed the almost universal plaints about the condition of modern education. While some of the criticism undoubtedly derives from a romantic nostalgia for a past that never existed, expressing something of the contemporary passion for fault-finding, there are, also, incontestably, very legitimate disappointments. The critiques first heard in the eighteenth century are being expressed again, more forcefully; they ought not to be ignored. Beyond all the achievements of specific disciplines—however commendable—a void is felt to exist. If the optimism of the seventeenth century is no longer possible, the explanation is not that war threatens or that bureaucrats are less hospitable to the support of scholarship than princes. The explanation may be quite different; it may have much to do with a shared intuitive sense that learning is a flawed modern enterprise. The most imperative need today may be to establish an agenda of research priorities that will sustain learning, in the tradition of scientific scholarship outlined by the essays in these *Daedalus* volumes, while seeking also a more explicit concern with education, broadly defined. The conceptualizing of what learning in a democratic society can be, a problem that did not exist for Bacon, but that is inseparable from what is happening in many parts of the world today, must figure prominently for those concerned with the advancement of learning in the last part of the twentieth century.

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SVETLANA ALPERS

Is Art History?

IT COMES AS NO SURPRISE to a student of art and its history these days to open a book on Italian painting and find an extensive discussion of barrel-gauging, or to turn to a study of Courbet and find many pages devoted to a detailed account of radicalism among French peasants in 1849 and 1850. The books by Michael Baxandall and T. J. Clark to which I am referring are not eccentric texts but among the most inventive and interesting studies of art written in recent years.[1] Distinctive though their emphases are, these writers share a commitment to consider the work of art as a "piece of history." Baxandall argues that we should consider Piero della Francesca's pictorial engagement with solid geometric forms in terms of the accepted fifteenth-century training in commercial mathematics. Similarly, Clark argues that an attention to the situation of French rural society enables us to understand the presence (in style, but also inseparably in content) of Courbet's great works of 1849–1850. I have chosen these two books as among the most rigorously argued of what is indeed a great number of such studies. It is a fashion by now, and almost established as one of the acceptable tools of the art historical trade. The new art history was announced in the title of a series of book-length studies of individual works initiated in the 1960s—*Art in Context*. The traditional mode of art history is represented by Pevsner's multivolumed *History of Art*, which began appearing in the 1950s and considers the history of art period by period, and country by country.[2]

What is worth remarking about the new look in the study of art is not its emphasis on art and society—for that has a long and somewhat checkered history—but rather the terms in which it is proposed. While previously it was the history of art, conceived in terms of the development and achievement of period styles, which was studied in the historical context (resulting in books like *Der Barock als Kunst der Gegenreformation*[3]), today it is individual works or groups of works, individual phenomena located at a particular time and place. Thus to amplify what I have just said: it is the work of art itself, not a history or sequence of works, which is seen as a piece of history.

A corollary to this change within the discipline is the phenomenon of historians turning to art not for confirmation of the notion of period style (one thinks for example of the use made of art in Friedrich's *The Age of the Baroque*[4]), but rather for the fact of individual works. It is a historian, Peter Gay, not an art historian, who most recently employed the phrase "piece of history" to describe

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the works discussed in his book on Manet, Gropius, and Mondrian.[5] The intellectual atmosphere is one in which historians frequently turn to works of art, and joint projects between historians and art historians flourish. Velázquez is being studied by such a team, as is the patronage of Julius II in Rome. (The nature of patronage is fast becoming a separate topic of study in the field of art history.) The sense of a common enterprise demonstrated in such projects is based on the assumption that the work of art, like any other event, is a piece of history.

The new social history of art as it is carried on by art historians concentrates on the circumstances of the making of an individual work. Who commissioned it, and where was it to be placed? What function (a central term here) did it serve and for what audience was it intended? Seen in this way documents establishing the commission and the later history or provenance of a work of art no longer testify to its pedigree but indeed to its very nature as an object. Specific methods of calculating the cost of paintings—so much per each full-length figure in the case of seventeenth-century Italian commission, for example—could determine, it is argued, the way a work looks. If, because of such considerations, a patron were willing to pay for only seven full-length figures in a picture of the Massacre of the Innocents, then Guido Reni, the artist in question, would have had to come up with his innovative reduced version of this traditionally many-figured scene. A particular compositional organization could be due to the position of the work, the actual site for which it was intended, and the angle from which it was to be viewed. Titian's removal of the Virgin from her traditional central position to the right side of the worshipers in his *Pesaro Madonna*—once considered a protobaroque stylistic invention—is now explained by the fact that the worshipers approached the work from the aisle to the left of the altarpiece. A revisionist interpretation of Michelangelo's Medici Chapel argues that it is less neoplatonic beliefs or stylistic concerns as such than the funerary function of the chapel and the specific liturgy for the dead composed for this place which were determining factors in the artist's inventions.[6] Almost unawares, such studies have come to a demystification of the notion of artistic invention. What was previously puzzled over as a mystery has now come to be understood as the task of fitting a work to a particular task, to a particular set of describable historical conditions. If a work of art is inevitably to be understood in terms of its particular historical circumstances, it is arguable that great art will result from a conscious working out of this recognition. Great art is, in short, in this essential way political in nature. However, those studies of Reni, Titian, and Michelangelo to which I have referred do not admit to this view. One of the things which I want to pursue later is the gap as I see it between the implications of this new social history and its acknowledged sense of itself and of art. In the name of clarity, rationality, and historical objectivity the new art history embraces a potentially radical view of art without accepting its implications.

Analogous to the demystification of artistic creation is Baxandall's bold attempt to demystify the problem of looking, of how we see. His study of quattrocento art addresses itself to how works of art were seen at a particular time by identifying habits of vision, modes of cognitive perception he calls them, as the social practices most relevant to the perception of paintings.

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Making what is a kind of social application of Gombrich's perceptual notion of the "beholder's share," Baxandall argues that eyes trained to gauge the volume of containers, to read the body language of sermons, or to respond in certain ways to color or its absence were catered to in just these terms by their artists. To quote him, "It is the highest common factor of skill in his public that the painter consistently catered for."[7] The delight in the complexity of volumetric forms in a Piero or Uccello, or the careful distinction made among various gestures in paintings of the Annunciation, is to be understood in this context. The appeal being made is to common experience, ordinary schooling one might say. Compare Baxandall's instance of barrel-gauging in discussing Piero della Francesca to the claimed artistic relevance of higher, more philosophical mathematics, invoked by Rudolf Wittkower in his landmark discoveries about Palladio, in *Architectural Principles in the Age of Humanism*.[8] Instead of the mathematics of the humanist we have that of the merchant. Piero himself is here not seen in conjunction with the Golden Section (frequently cited in analyses of the composition of his *Flagellation*), but with high school mathematics for which, as Baxandall points out, the artist himself wrote a text.

Despite a continuing concern to deal with major works, the view of art put forth in these studies is basically a leveling one. And in fact if we look at the broad spectrum of phenomena that are being studied today a leveling tendency is very much in evidence. Wölfflin, in a famous and oft-repeated passage, once argued the stylistic equivalence between Gothic shoes and Gothic architecture.[9] It was in the smallest shapes, he said, that the purest manifestation of a style can be felt. It is thus in the grand order of styles that shoes are the equal of cathedrals. While Wölfflin treated shoes on a par with art, today shoes and cathedrals, but more to our point barns and cathedrals, carpets and frescoes are all treated as products of society. The studies of so-called vernacular architecture (like barns) no longer seem eccentric in an atmosphere in which architecture can be defined not in terms of monuments but as any changes at all that man makes in his environment. Apartment houses and tenements take their place beside town houses and public buildings in this enterprise. In a curious meeting of the old and the newest, Oleg Grabar's article, "An Art of the Object" (occasioned by the Metropolitan Museum's opening of its Islamic installation) brought oriental carpets and designs into the pages of *ARTFORUM*.[10] This publication testifies to the modern blurring of the line between art and craft, seen also in the acceptance of pottery and weaving among the media of art and institutionally in the interest in the historical artifacts being exhibited by newly invigorated historical museums all over the country. But it also testifies to the extension of serious study to the "decorative" (the term is no longer pejorative) traditions of non-Western art. We should be reminded of Riegl's arguments of seventy-five years ago which used the traditional motifs in textiles as a model for the history of style.[11] However, as we saw in the comparison with Wölfflin, there is today no appeal to a higher and more extensive historical reality such as style. It is rather their common functions as social products that bind these various artifacts together.

There is further a healthy decentralization in the view of art, what art is, where and by whom it has been and is produced. More kinds of objects, but also more kinds of makers are being admitted into the circle of artists. Women,

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traditionally makers in the household, are a significant group. From studies of art in China and India to Europe there is a similar scholarly attention to the status and working conditions of artist-craftsmen and their influence on the nature and reception of works.[12] When and where were artists born into professional families? When and where were they treated as craftsmen or honored as members of wealthy households? Consider the double or triple professions of many Dutch artists in the seventeenth century: Steen was a brewer as well as a painter; Vermeer was probably both art dealer and innkeeper. This now seems less a case of an underdeveloped country, by comparison with Italy, than an example of a different social and thus artistic situation. The net in which we catch art and artists is spread widely and much more is caught for our consideration. The question, "What is art?" and the old answer that defined it as having aesthetic rather than utilitarian appeal never seemed further from anyone's mind.

One way in which to understand the directions being taken today in the study of art is to see them as offering alternatives to a dominant mode of the first great generations of modern art historians. The most influential figures among these, Wölfflin for example, and Panofsky, largely concentrated their attention and developed their modes of analysis with reference to the art of the Italian Renaissance. Both because of Panofsky's influence inside and outside the field, and because of the explicit way in which his writings lay out his assumptions, I have chosen to let him serve as the example of a general phenomenon. Panofsky's method of iconographic analysis originated in a series of primarily theoretical papers. His iconographic method, loosely construed, has been adopted by students of the pictorial (and verbal) arts of all ages. But Panofsky's interpretive strategy was bound up with Renaissance phenomena and with the study of Renaissance art. In his early papers (which remain untranslated) Panofsky argued against trying to decipher meanings using the notion of style, which was then defined psychologically by Wölfflin as "forms of beholding" (*Sehformen*) or by Riegl as the sociopsychological *Kunstwollen* (variously translated "will to form," "artistic volition," or more literally "that which wills art").[13] Arguing instead for the way in which pictorial images are bound to ideas, Panofsky defined their essential meaning in terms of *Sinn*, later rendered as "intrinsic meaning" in his English publications. Adopting Cassirer's notion of symbolic forms, the great achievement of Panofsky's approach was that it drew attention to the links between pictures and ideas, between art and thought. But whether he is defining the Renaissance as the reintegration of classical motif with classical theme, or pointing to the symbolic force of the use of perspective construction in Renaissance painting, Panofsky in effect posits and confirms a particular notion of man and of art.[14] Man here is a figure located historically in time (acknowledging his historical relationship to antiquity) and in space (employing the device of linear perspective to posit his picture as a window onto a second, substitute world). Man is at the center, viewing the world beyond of which his art is the imagined imitation. The very notion of the viewer's relationship to meanings which are verbally construed and which lie beneath the surface of his meaningful images, is bound up with Renaissance notions of art and the primacy of language. While validating and expounding a certain view of man, and the kind of art he produced, Panofsky was clear at least at first, that this was

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distinguished from what came before (Middle Ages) and what came after (the modern age).

> But no mediaeval man could see the civilization of antiquity as a phenomenon complete in itself, yet belonging to the past and historically detached from the contemporary world—as a cultural cosmos to be investigated and, if possible, to be reintegrated, instead of being a world of living wonders or a mine of information. . . . Just as it was impossible for the Middle Ages to elaborate the modern system of perspective, which is based on the realization of a fixed distance between the eye and the object and thus enables the artist to build up comprehensive and consistent images of visible things; just as impossible was it for them to evolve the modern idea of history, which is based on the realization of an intellectual distance between the present and the past, and thus enables the scholar to build up comprehensive and consistent concepts of bygone periods.[15]

It is when Panofsky's method was expanded, by him and by the field in general, to other arts and then treated as a normative standard for art and its study that problems developed. Compelling though his study of early Netherlandish painting is, it more and more seems to me that it compels partly by making northern art follow the art of the south.[16] Northern artists, far longer and more persistently than those in the south, trusted to the flatness of the working surface. Although it is true that the illuminated pages of books became more and more picturelike in the Italian Renaissance sense, the flatness of the surface of northern art in general was not killed off by perspective as Panofsky suggests. Neither the flat surface nor the repetition involved in printing techniques disturbed northern European artists, who, unlike the Italians, did not accept the authority of the individual work, created by an individual artist for a located viewer. As an image of the world, northern art is often more like a mirror than like a window. This is true of Van Eyck as it is of Vermeer. The common denominator between both aspects (flatness of working surface and mirror) is the absence of the maker or viewer, clearly posited in space and located in time, that is central to the creation of Italian art. Northern art, it might be argued, moved from the eye of God (Van Eyck's mirror in the *Arnolfini Wedding*) to the optical lens like a reflecting eye (the world cast onto the surface of the canvas as in Vermeer). But all the time a human center, in Panofsky's Italian Renaissance sense, is strangely absent. Finally Panofsky's definition of disguised symbolism—his inventive way of analyzing the combination of new pictorial realism and old medieval symbolism in the art of the north—in effect follows Italian fashions by subordinating surface imitation to meaning. He does not allow for the fact that the appearance of the first seems so often to outrun the verbal presence of the second, even in Van Eyck.

If we turn to Panofsky's masterful study of Dürer, it is characteristic that he sees Dürer as a kind of captive of the alien northern darkness struggling toward the southern light.[17] This of course is how Dürer often saw himself and it is thus in keeping with much of his art. But it is less clear that there is a right and a wrong, a light (the south) and a darkness (the north) here. In view of the accomplishments of Dürer's art one might says that he was far better at a characteristically northern task, print-making, than at a southern one, painting. Or to pursue the contrast further, that his landscapes (northern) are far superior to his nudes (southern).

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As heirs to this southern emphasis, students of the northern tradition have until recently been inordinately interested in those artists and works which are bound in some way to the south of Europe. Motifs and meanings have been tracked down with great energy. Despite the evidence of the pictures themselves, an entire moralistic school of interpretation of Dutch seventeenth-century realism has developed on the assumption that hidden meaning, not the surface, is the key to pictorial significance. Despite their pictorial balance and presence, Vermeer's women, for example, are seen as emblems of sin. Although moralizing emblem books were popular in the Netherlands and some paintings followed suit, the impetus for this kind of interpretation can be explained at least in part as an attempt to lend the right kind of significance to an otherwise—by Italian standards—insignificant art. This attitude is widespread. Lord Clark introduces his book on *Rembrandt and the Italian Renaissance*[18] by asking how such an artist could rise from the mediocrity of Dutch art. And he goes on to expound Rembrandt's greatness in terms of Italian models. He thus speaks from a specifically Italian notion of greatness and incidentally leaves precious little room for the more obviously native achievements of a Saenredam, a Ruisdael, or a Vermeer.

Although he is suspicious of subjectivity, Panofsky's view of art and its history is clearly on the side of certain values, certain notions of man, art, and history for which he claims objective validity. Thus although his methods for studying art and its history were historically located as appropriate ways of dealing with Renaissance art, he came to treat them as normative and so they came to be seen by the discipline itself. In the course of his well-known "The History of Art as a Humanistic Discipline," Panofsky makes the statement that man is "fundamentally an historian" and that "to grasp reality we have to detach ourselves from the present."[19] He speaks as a Renaissance viewer, located in time and space, detached from what he observes as he is from the process of observing. The very title of this essay, which long has been given to our students as a standard definition of and defense of the study of art history, can now be seen as grounded in Renaissance assumptions about man and art. What happens, one wonders, if the art one wants to study is itself not based on such humanistic principles? Is it not art? Can it not be studied by art historians?

These questions lead us back to the concerns occupying students of art today. My argument is that much of what was previously considered marginal—in terms of kinds of objects, whole civilizations, but also in terms of the questions one would ask about them—is presently holding our attention. In effect, then, questions about our assumptions are raised by what is being done in the study of art as a piece of history. I say "in effect," however, because they are hardly being voiced as questions except by Marxist critics.[20] To them, nothing that I have said will seem new, and much of what I have said will seem mild or too superficial in ideological terms. To ask why such questions are not being addressed is to speak not only to the inertia inherent in any academic discipline, but also to strike to the heart of the discipline's sense of itself as a discipline. For as scholars, art historians all too often see themselves as being in pursuit of knowledge without recognizing how they themselves are the makers of that knowledge.

It is often in recognizing the assumptions underlying one's research, one's

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habits of mind in a sense, that new problems are clarified and new issues are made clear. If we recognize our current intellectual stance as a challenge to the previous hegemony of the Renaissance, we should go on to reconsider some things that we have made basic to the study of art. Three issues, basic operating procedures actually, built right into our study of Western art come to mind: (1) the notion of the role or the authority of the individual maker; (2) the notion of the uniqueness of the individual work; and (3) the notion of the centrality of the institution of painting.

It is common procedure to begin any study by attributing and dating the works to be considered and separating them out from any possible imitations. On what basis (other than market value) must we consider the authority of an individual maker as the central feature of every work? Entire modes of art which we are now beginning to include in our studies do not depend on such identification. The assertion of the identity of the maker is properly studied against the background of a much larger production of anonymous objects which were made apart from such a recognition of self. Perhaps there was a conscious attempt at self-effacement, an attempt to blend into an admired style or mode of image-making. In studying a tradition such as Chinese painting, where imitation of an admired style is the rule and attribution a chancy and demanding procedure at best, would it not be useful to ask how appropriate the task of attribution is? How is the individual maker related as an individual to such a powerful and absorbing tradition? How should we deal with the collaborative effort of workshop products, such as medieval illuminated manuscripts or Renaissance frescoed rooms? Should our aim always be first to sort out, to identify the hands? What is the status or nature of collaborative efforts at different times, in different societies? What were the conditions of working together?

Turning to the works themselves, our assumption about the absolute uniqueness of the original work is a counterpart to this notion of individual creators. There are first of all types of objects—prints, but also tapestries or photographs—which are designed to be replicated. Not only does our current method of print connoisseurship lead us, against this very fact, to continue to sort out when and in which order each individual pull of a print was made, but further the very notion of the value of repetition is hardly faced at all. If as William Ivins has argued in his feisty but powerful *Prints and Visual Communication*, one function of a pictorial image is the communication of information (as in a map, or the illustrations to a study of botany, both of which were worked on by Dutch artists in the seventeenth century) then the possibility of repetition is a prime virtue, not a vice.[21] That Rembrandt fought this possibility, often creating essentially unrepeatable etchings, is a different but not necessarily on this account (though it may be on others) a superior artistic achievement. There are entire schools of art, such as the Dutch, or artistic enterprises, such as Monet's series of haystacks or poplars, which might be better understood in the light of a more general appreciation of repetition. The strength of the hold which the original (in the sense of the originating or first in a sequence of inventions) has on us is made clear even in such an independent study as George Kubler's *The Shape of Time*. Kubler begins by proposing that "the idea of art can be expanded to embrace the whole range of man-made things."[22] He proceeds

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to search for an order in which to put all these things and insists on establishing distinctions between what he calls "prime objects" and "replicas." In arguing, for example, that "with European objects we often come closer to the hot moment of invention than in non-European ones where our knowledge is so often based only upon replicas of uniform or debased quality," he seems to me to reveal a European cultural bias (but of course also an appropriate cultural responsiveness) which we are just starting to reflect on as we expand the bounds of art and its history.[23]

As a final example of new directions in which to turn, consider the notion of painting as an institution—I mean the sense in which in our study of Western art a new subject, like landscape, is considered to have truly arrived only when it is rendered in the most permanent and expensive media, paint. One notes the ease with which students of Western painting commonly speak of sources in a minor medium such as prints, as if they were there just to serve painting. It is as if the transition from one medium to another was not in itself problematic and worthy of assessment. Gombrich, for example, has demonstrated most eloquently that it is not a new look at the actual landscape on the part of painters in the north of Europe, but rather a theory making landscape a suitable subject for art contributed by southern writers that led to the establishment of the new genre of landscape painting. "Here then," he writes, "was a frame into which the admired products of northern skill and patience could be fitted."[24] And he goes on to discuss their classification in terms of heroic and pastoral types. Yet as Gombrich himself admits a few pages earlier, citing the example of Dürer, northern skill and patience had already flowered. For Dürer was indeed "one of the world's greatest landscape painters" though in his topographical watercolors done for his own pleasure, not in paintings for purchase.[25] Is the topographic watercolor not art? And when we look at the great northern landscape painters—Bruegel, and then Van Goyen and Ruisdael—is it true that topography could not be the motif or function for paintings (as contrasted with a watercolor)? Is the birth of landscape painting in the north so at variance with the function of the lesser media? What if the entire tradition of northern art is indeed rather like painted prints? Is the human measure—what Gombrich means by the institution of landscape as a type of painting—essential before pictorial renderings can be considered in the realm of art? We have here indeed the recipe for the makings of Renaissance art, but surely not for all picturing.

Far from being limited to a revisionist study of art, the questions we are touching on here, the very mode of thinking that leads me to pose them, is shared by many thinkers today. The status or nature of the individual creator is, for example, a central concern in all of the works of Michel Foucault.[26] His account of what he terms the archeology of knowledge emphasizes, like Braudel's study of the Mediterranean, what Braudel calls "that other submerged history, almost silent and always discreet, virtually unsuspected either by its observers or its participants."[27] This view of the human situation, the slow process of continuing transformation enacted between us and our environment(s) is also confirmed in much contemporary art. We might take as a prime example the writing and the earth-works of the late Robert Smithson—be it his evocation of Olmsted's Central Park as an "ongoing development" of slow geological changes interacting with an environment for man, or his *Spiral Jetty* made out of, and subsumed once again into, the Great Salt Lake.[28]

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To ask these questions is then to acknowledge a common enterprise with historians, critics, artists, and writers of our time, and one would think, to gain strength and insight from such a common intellectual cause. From this vantage point we can now return to our earlier remarks on Panofsky and to his generation of art historians in a slightly different light. For in marked contrast to the generation of Panofsky art historians today, far from acknowledging that they live and work at a certain time and place, in the company of others (all of which would be considered subjective factors) deny this in favor of a notion of objective historical research. Baxandall, a most sophisticated spokesman, argues that "quattrocento intentions happened in quattrocento terms, not in ours."[29] It is characteristic of art history that we teach our graduate students the methods, the "how to do it" of the discipline (how to date, attribute, track down a commission, analyze style and iconography) rather than the nature of our thinking. In terms of the intellectual history of the discipline our students are woefully uneducated. How many have been asked to read Panofsky's early untranslated writings, or Riegl, or Wölfflin? Supporting this is that prejudice for the original object and against the desk-bound scholar. To think, to write is itself somehow to forsake the works. At issue is not the method one uses but rather the notion of art and its history, the notion of man and the form that his knowledge of the world takes. For all of Panofsky's claims to employing an objective method (his three levels of meaning, for example) he accepted the responsibility for his own thought and his commitment to certain values. In studying Renaissance art he was aware of having made certain choices; he was aware of the phenomena that he excluded by making them. He celebrated the accomplishment of a humanistic art and despaired at the loss of it. A certain latitude was possible within this set of mind. Panofsky celebrates what he saw as a kind of cultural wholeness testified to in the symbolic content of Renaissance images. Aby Warburg, more psychological in his sensibility, studied that precarious balance among competing human impulses that is achieved by a society in the ordering of its rituals (the Navajos served him as an example) or by an individual in his patronage of art. In the midst of the current rush of alternatives to an earlier mode of studying art, it is only the radical critics of the field who engage those issues of society and history, those questions of moral values that were, by earlier scholars, accepted as an essential part of their scholarship. To what view of human and societal values, and to what understanding of the sequence of objects that we call history do we tend in our current mode of equating all works as separate but equal pieces of history?

One of the few people to see and address this as an issue has been E. H. Gombrich. Best known for his studies of representation and artistic illusion and the perceptual processes at work in receiving and forming images of the world, Gombrich has devoted himself on a number of recent occasions to the problem of maintaining the values of tradition.[30] His long-standing critique of the notion of *Zeitgeist* is extended here in an argument directed against the social historicization of the study and perception of art and the relativistic view of all values that results. On the one hand Gombrich is firm in his belief that a disciplined study of what he calls "the psychological and technical resources of the visual media" is necessary. But he also speaks to the value of personal response, which, with a characteristic confidence in right-mindedness, he feels will inevitably support traditional canons of taste, support in short the value of tradition itself.[31]

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Gombrich's concern with the perceptual processes of vision can be easily accommodated to artistic notions of modernism, even as his analytic rigor can be accommodated to current structuralist modes of analyses. In contrast to modernist art and thought, however, he insists that our visual perception, like our judgment, is validated by external phenomena. For Gombrich the "beholder's share," that happy phrase by which he evokes the part we play in perceiving any representative illusion (or by extension one might say make a judgment), depends on a notion of constancy, constant objects in the world, constant values posited outside ourselves. This is, as Gombrich has often argued, simply a matter of common sense.

But it is common sense—taking this to refer to a community able to agree on certain realities and certain values—that is today being challenged by experience and by research. Many do not share Gombrich's confidence in tradition. Those who do, do not share Gombrich's urgent sense that it needs reiteration. Some of the most interesting work is being produced out a sense of this situation. The studies by Leo Steinberg, Michael Fried, and T. J. Clark that I particularly have in mind are perhaps more accurately called writings. For they are all acts of writing as much as they are reports of research accomplished.[32]

These three scholars differ from one another greatly in subject, attitude, and manner of address. But they share a meditative stance which testifies in each case, I think, to work which is not only reflective in nature but which might also properly be called reflexive. Each one clearly establishes where he stands. It is useful to recall that several years ago in these pages, Leo Steinberg argued eloquently against what he called the rule of objectivity and the shrinking self.[33] For Steinberg the self is a psychological presence, for Michael Fried it is rather a beholder, in a more philosophical sense, of pictorial fictions or images, whereas for T. J. Clark the self is a member of a particular society and class.[34] To the sensible question of whether it is appropriate to deal with Michelangelo, say, or Greuze or Courbet in terms of viewers so conceived, the answer these scholars seem to give is some mixture of evidence from the lifetime of the artist and from continuing human necessity. Surely Baxandall's disembodied desire—one shared by many art historians today—to see quattrocento art in quattrocento terms is as much an intellectual structuring of our time as the others. This too is knowledge we make.

Of these three writers, T. J. Clark comes closest to providing a response to the present problematic of our field as I have described it here. To repeat something said earlier, if art is most powerfully understood in its particular historical circumstances, then it is arguable that great art would result from conscious working out of this recognition. It is this claim that Clark makes for the art of Courbet in the years 1849–1850. Clark's *Image of the People* leads us with care through the social and political upheavals in France during these years. He traces Courbet's roots in the fractured and radicalized world of rural Besançon, his experimentation with popular imagery and popular forms taken from the example of simple popular engravings until Courbet finally comes to a pictorial definition of his audience. In Clark's analysis, the *Burial at Ornans* subverts the expectations of the normal public for high art by filling a work of monumental proportions and presumptions with the disorderly actualities of village society. In contrast to the traditional treatment of Courbet as a realist,

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his art is here not only seen as a piece of history but is shown to have been made as such. One of the virtues of this study is that it understands its assumptions. Clark argues for the ideological determinants of art and he attempts to locate these in the complex surface, the figures, but also the very colors and brush strokes of a work. Let us extract some sentences from Clark's description of the *Burial at Ornans*.

> He has painted more than forty-five figures life-size in a great frieze over eight yards long, arranging the figures in a long row which curves back slightly round the grave itself; and in places, following the conventions of popular art, he has piled the figures one on top of the other as if they stood on steeply sloping ground. And towards the right of the picture he has let the mass of mourners congeal into a solid wall of black pigment, against which the face of the mayor's daughter and the handkerchief which covers his sister Zoë's face register as tenuous, almost tragic interruptions. He has used colour deliberately and dramatically, in a way which has little to do with the careful materialism of the *Stonebreakers*, to symbolize matter; almost, as our eyes move right, to threaten the faces put upon the solid ground. . . . Beyond this point, when we start to ask about the picture's meaning, the real difficulties begin. What, to put it briefly, is the *Burial*'s affective atmosphere? What are the mourners' attitudes and emotions, and what is Courbet's attitude to the event portrayed? . . . We have to answer such questions in the face of an image which deliberately avoids emotions organization: by that I mean the orchestration of forms to mimic and underline the emotional connotations of the subject. . . . Is the *Burial* a sacrament or merely a social occasion? . . . It was precisely its lack of open, declared *significance* which offended most of all; it was the way the *Burial* seemed to hide its attitudes, seemed to contain within itself too many contraries—religious and secular, comic and tragic, sentimental and grotesque. It was this inclusiveness, this exact and cruel deadpan, that made the *Burial* the focus of such different meanings. It was an image that took on the colours of its context; and perhaps it was designed to do so.[35]

But despite all this there are things that are left out. Clark does not intend a traditional assessment of the oeuvre of a master. The exclusions that he makes—many other works, but also other aspects of those he chooses to discuss—are made consciously. One can understand why they do not appear here. Clark can tell us of the social function of the *Burial at Ornans*, but what about Courbet's extraordinary self-portraits? Surely a discovery of self went along with Courbet's discovery of the state of his society when he returned to his village birthplace to paint in 1849–1850. And what if the issues are not self and society? How are we to understand that he painted landscapes and still-lifes as he did?

The importance of Clark's work, like Steinberg's and Fried's, is not to demonstrate the strengths or the limits of a particular approach. It is rather that these writings set a standard, offer a level of thinking, of looking, of pictorial analysis that we in art history sorely need. In distinguishing the preeminent problems in our field today, the criticisms that I offered about how they are being dealt with are less criticisms of the particular assumptions made, than criticism of assumptions not perceived, not acknowledged. The pressing need in other words is to recognize just what it is that we are about.

It is curious to note that each of these three very different scholars has been at pains to give an account of the nature and attentiveness of past viewers of the works that they are studying. Steinberg characteristically sets his studies in this very frame, summarizing for us how a work—Michelangelo's *Last Judgment*, for

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example—has been seen through the ages. Fried in studying the critiques of the eighteenth-century French salons and Clark in studying the reactions to the exhibitions of Courbet's works of 1849–1850 have made the reactions to the basic material of their research and analysis. More important than the distinctiveness of their approaches (Steinberg might be called a psychoanalytic, Fried a formalist, and Clark a Marxist critic) is the common claim made by these scholars, against the evidence of most art historical writing today, that not only research *about*, but looking *at* a work, takes time. They all show that it took time to look in the past and they offer us ways in which it can today.

In the greater expanse of art history this fact has frequently been lost sight of, though indeed in reading Riegl, Wölfflin, Focillon, or Lawrence Gowing, for example, we find writers who did not. But it is a particularly pressing issue today in the atmosphere and with the kind of intellectual engagements outlined in the opening part of this essay. With such a profusion of objects and cultures, with old hierarchies crumbling, how does one justify such an occupation as looking? It is a daunting question.

References

[1]Michael Baxandall, *Painting and Experience in Fifteenth Century Florence: A Primer in the Social History of Style*(London, 1972) and T. J. Clark, *Image of the People: Gustave Courbet and the 1848 Revolution* (London, 1973). Two remarks should be appended by way of introduction to this essay. First, although the examples of both present and past work in art history will come largely from those areas I know best, the Renaissance and after, I think that the points I shall make are not limited to these areas. Second, in choosing to emphasize the directions being taken by innovative work I do not mean to deny that much excellent scholarship of a more traditional kind continues to be done.

[2]John Fleming and Hugh Honour (eds.), *Art in Context*; Nikolaus Pevsner, *The Pelican History of Art* (London, 1953–).

[3]Werner Weisbach, *Der Barock als Kunst der Gegenreformation* (Berlin, 1921).

[4]Carl J. Friedrich, *The Age of the Baroque* (New York, 1952).

[5]Peter Gay, *Art and Act: On Causes in History—Manet, Gropius, Mondrian* (New York, 1976), p. 3.

[6]I am referring here to the following studies: Francis Haskell, *Patrons and Painters* (London, 1963), p. 10 n. (Reni); David Rosand, "Titian in the Frari," *Art Bulletin*, 53 (1970): 206 (Titian); L. D. Ettlinger's as yet unpublished study of Michelangelo's Medici Chapel.

[7]Baxandall, *Painting and Experience in Fifteenth Century Florence*, p.40.

[8]Rudolf Wittkower, *Architectural Principles in the Age of Humanism* (London, 1949).

[9]Heinrich Wölfflin, *Prolegomena zu einer Psychologie der Architektur* (1886), reprinted in *Kleine Schriften* (Basel, 1946), see p. 44.

[10]Oleg Grabar, "An Art of the Object," *ARTFORUM* (March 1976): 36–43.

[11]Alois Riegl, *Stilfragen* (Berlin, 1893).

[12]To give but two examples in the work of colleagues at Berkeley: James Cahill's recent study (unpublished), "Life Patterns and Stylistic Directions in Ming Painting," and Joanne Williams, "Caste and the Role of the Painter in Mughal India" (also unpublished).

[13]Erwin Panofsky, "Der Begriff des Kunstwollens," *Zeitschrift für Aesthetik und Algemeine Kunstwissenschaft*, 14 (1920); 321–339.

[14]See Erwin Panofsky, "Iconography and Iconology: An Introduction to the Study of Renaissance Art," *Meaning in the Visual Arts* (Garden City, 1955), pp. 26–54.

[15]Erwin Panofsky, "Introductory," in *Studies in Iconography: Humanistic Themes in the Art of the Renaissance* (New York, 1962), pp. 27–28. First published in 1939.

[16]Erwin Panofsky, *Early Netherlandish Painting, Its Origins and Character* (Cambridge, Mass., 1955).

[17]Erwin Panofsky, *The Life and Art of Albrecht Dürer* (Princeton, 1955). First published in 1943.

[18]Kenneth Clark, *Rembrandt and the Italian Renaissance* (New York, 1966).

[19]Erwin Panofsky, "The History of Art as a Humanistic Discipline," printed as the Introduction to *Meaning in the Visual Arts* (Garden City, 1955); see pp. 5 and 24 for the quotations cited.

[20]There are of course some exceptions. Early on James Ackerman voiced concerns about the direction being taken by art history in "Western Art History," in *Art and Archaeology* (Englewood Cliffs, N.J., 1963), where he pointed to the noninterventionist stance of American art historians. He

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has not been alone among art historians since then in turning to the film (Ackerman has even made one) by way of being more in touch with the realities of modern society. In this connection we should remember that Panofsky himself wrote a piece welcoming and, in certain respects at least definitively defining, this newest of artistic media. As always Panofsky located just where he stood in relationship to it: "It is the movies, and only the movies that do justice to that materialistic interpretation of the universe which, whether we like it or not, pervades contemporary civilization." "Style and Medium in the Motion Pictures," *Critique*, 1, 3 (January-February, 1947), reprinted in Gerald Mast and Marshall Cohen (eds.), *Film Theory and Criticism* (New York, 1974), pp. 151–169).

[21]William M. Ivins, Jr., *Prints and Visual Communication* (Cambridge, Mass., 1968). First published in 1953.

[22]George Kubler, *The Shape of Time: Remarks on the History of Things* (New Haven, 1962), p.1.

[23]Ibid., p. 39 ff. and p. 44.

[24]E. H. Gombrich, "The Renaissance Theory of Art and the Rise of Landscape Painting," in *Norm and Form* (London, 1966), p. 114.

[25]Ibid., p. 108.

[26]See particularly Michel Foucault, "What is an Author?" *Partisan Review* (1975): 603–614.

[27]Fernand Braudel, *The Mediterranean*, tr. Sian Reynolds (New York, 1973), vol. 1, p. 16.

[28]Robert Smithson, "Frederick Law Olmsted and the Dialectical Landscape," *ARTFORUM* (February, 1973): pp. 62–68.

[29]Baxandall, *Painting and Experience in Fifteenth Century Florence*, p. 151.

[30]For example, see E. H. Gombrich, "Art History and the Social Sciences," *The Romanes Lecture for 1973* (Oxford, 1975).

[31]These comments are based on Gombrich's Erasmus Prize acceptance speech as published in *Simiolus*, 8 (1975–1976): 47–48.

[32]Leo Steinberg has published books, articles, and reviews on many different areas of art. See his *Other Criteria: Confrontations with Twentieth Century Art* (New York, 1972), or more recently *Michelangelo's Last Paintings* (Oxford, 1976). Michael Fried, first known for his studies of contemporary artists, has also written on nineteeth-century French art and is now studying its antecedents in the eighteenth century. See his "Absorption: A Master Theme in French Painting," *Eighteenth Century Studies*, 9 (1975–1976): 139–177; and "Towards a Supreme Fiction: Genre and Beholder in the Art Criticism of Diderot and His Contemporaries," *New Literary History* (Spring, 1975): 534–584. Besides his study of Courbet mentioned earlier, T. J. Clark has also written a book on Daumier, *The Absolute Bourgeois* (London, 1973), and has now turned to Impressionism and after.

[33]Leo Steinberg, "Objectivity and the Shrinking Self," *Daedalus* (Summer, 1969): 824–836.

[34]A specific woman's sense of self is also being articulated in art studies today. The ground for such writing was laid (was in effect cleared) by Linda Nochlin in an article first published in 1971, "Why Have There Been No Great Woman Artists," reprinted in Thomas B. Hess and Elizabeth B. Baker (eds.), *Art and Sexual Politics* (New York, 1973). It has been part of the groundswell of revisionist views of art history. If I have not selected out any single woman writer here, it is because it seems to me to be more a chorus than distinct individual voices. A major virtue of the woman's movement in art is that the chorus joins art historians with critics and artists in an easy relationship not usual in these tight professional worlds. This promises much.

[35]T. J. Clark, *The Image of the People*, pp. 82–83.

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Elementary Particle Physics

THE ELUSIVE QUARK has emerged during the past decade as the newest candidate for the most elementary constituent, or ultimate building block, of Nature. For example the proton, which is the nucleus of the hydrogen atom, is believed to consist of three quarks bound together by a strong force. However, not a single quark has ever been detected in the laboratory in spite of many efforts to find one. In addition to lacking proof of their existence by a direct laboratory observation, we also lack a fundamental theory of quarks. Among the unanswered questions are: How many different kinds of quarks are there? Do they have an internal structure of their own? What forces bind them to one another to form protons and the other observed subnuclear structures? Can these bonds ever be broken so that isolated quarks can be observed?

In spite of this profound and extensive state of uncertainty the quark concept has proved to be very useful in our search for an underlying unity to the diverse phenomena observed in today's world of elementary particle physics as we probe in the laboratory on scales of distance that are tens to hundreds of millions times smaller than the size of what we now call the "atom," and on scales of time that are shorter than one hundredth of one millionth of typical atomic time scales. And along with the quarks, new ideas of elementary particle structure have been proposed. Foremost among these is the possibility that the individual quarks, although viewed as elementary constituents, have not been seen so far because, in principle, they may never be isolated one at a time. By turning to the concept of a quark thus shrouded in mystery in its search for basic building blocks, modern science appears to be turning its clocks back to the time of the Greek philosophers of 2500 years ago, and turning its back on the traditions and methods of more than five centuries, which have rooted nature's laws in observable concepts.

The newest ideas on the frontiers of the submicroscopic unknown and the radical change in our concepts from the early Greeks to today's quarks are the subject of this essay. This is an especially hazardous time for such an undertaking. We have just passed through two highly explosive years of progress in the field of elementary particle physics and the new discoveries not only have not been fully understood; they are still occurring. With detectors of improved sensitivity and with facilities of greater resolving power new frontiers of the unknown are being explored. Perhaps individual quarks will be observed directly, perhaps not. Perhaps new species of quarks will have to be introduced,

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perhaps not. No matter what the results, the recent momentous experimental discoveries and theoretical successes have generated considerable optimism that quarks represent a major stride forward in our understanding of the basic structure of nature.

If, in the future, individual quarks are observed directly in the laboratory they will assume a respected and more traditional role as the next hierarchy of elementary building blocks. If, on the contrary, individual quarks remain unobservable as basic building blocks of nature, no matter how hard we look (a result predicted by some of the most attractive current theoretical hypotheses) our view of submicroscopic nature will undergo a major revolution. The last such major revolution occurred earlier this century with the formulation of the quantum theory of atomic phenomena in 1925–1926.

"What Are We Made Of?"

Since the beginning of recorded history man has asked, "What are we made of?" Science first flourished twenty-five hundred years ago with the search of the early Greek philosophers for the answer to this question. Their search for an underlying unity to the rich diversity observed in the world around them led beyond appearances, for, as Aristotle wrote a century later: "So we must advance from the concrete whole to the several constituents which it embraces; for it is the concrete whole that is the more readily cognizable by the senses. And by calling the concrete a 'whole' I mean that it embraces in a single complex a diversity of constituent elements, factors, or properties."[1] Once beyond appearances, different Greek philosophers moved in various directions in postulating the invisible constituents or qualities out of which all else is constructed. According to Democritus, who was the most important founder of the atomic theory, the world is built of *indivisible* constituents, or *atoms*. The atoms are simpler than all the rich, varied phenomena we see around us, but by their motion and behavior they, though not directly observable, control all we do see. (In fact Democritus wrote that, apart from free atoms and empty space, all else in the world was opinions![2]) The realization that the search for nature's fundamental building blocks and basic processes carries us beyond the sense world of appearance into an atomic realm was a major advance in man's view of nature.

This search for a unity underlying the diversity observed in nature was to the early Greeks a purely theoretical exercise of the intellect. They did not insist on seeing their atoms. It was a matter of debate whether ultimate atoms existed in the mold of Democritus, or whether matter could be continually subdivided, as envisioned by Anaxagoras, into seeds within seeds within seeds ad infinitum.[3] However, modern science recognizes that logic alone cannot answer which, if indeed either, of these views is consistent with nature. Above and beyond the metaphysician, experiments and data are required; and, beyond the data themselves, laws with predictive power and subject to experimental testing must be constructed by a process of abstraction and generalization. In the words of Niels Bohr, one of the most important architects of modern atomic theory, "Only by experience itself do we come to recognize those laws which grant us a comprehensive view of the diversity of phenomena."[4] This is the scientific process as developed and applied for the past five centuries.

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Atomic Measurement

In contrast to the early Greeks and in accord with the words of Bohr, modern science has insisted on detecting the so-called elementary particles which are the building blocks of matter. The particles' traces are identified from the tracks and signals which they form in specifically designed detectors. Early in this century the remarkable art of experimentation developed to the extent that it was possible to study the properties of individual atoms. As a result of this fantastic sensitivity of measurement our entire concept of observation underwent revolutionary changes. On the atomic frontier the most profound and radical change occurred when it was realized that it is necessary to take into account the effect of the observation itself on the physical system being observed. The interaction between observer and object can be determined with very high accuracy or even ignored as negligibly small for measurements on the macroscopic world. This is not so, however, for observations of individual atoms.

A recognition of the measurement process itself and an understanding of its inevitable and not entirely predictable effect on what is observed formed the foundations of the modern quantum theory. As Heisenberg, the father of the quantum theory, has written:

> . . . the interaction between observer and object causes uncontrollable and large changes in the system being observed, because of the discontinuous changes characteristic of atomic processes. The immediate consequence of this circumstance is that in general every experiment performed to determine some numerical quantity renders the knowledge of others illusory, since the uncontrollable perturbation of the observed system alters the values of previously determined quantities. If this perturbation be followed in its quantitative details, it appears that in many cases it is impossible to obtain an exact determination of the simultaneous values of two variables, but rather that there is a lower limit to the accuracy with which they can be known.[5]

Along with this recognition of a fundamental limitation in principle to the accuracy with which measurements of atomic phenomena can be made, it became necessary to introduce probability notions and to resort to wave as well as particle concepts to describe the observed properties of electrons and atoms. Classical particle concepts and causal connections from the macroscopic world of experience proved inadequate to describe the observed properties and interactions of electrons and of atoms. As a result of fundamental limitations of the measurement process, a major revolution occurred in man's concept of the elementary particle which could no longer be described by concepts of classical mechanics alone. But there is no uncertainty in what is meant when we say that we *observe* an electron as an elementary constituent of the atom.

Now, fifty years later, with the vastly increased sensitivity of the new particle detectors and with the vastly increased resolving power of the great accelerators, we have advanced our elementary particle frontier onto a scale of distances a million and more times smaller than the atomic scale, and we have come upon the quarks. And once again we are facing a major revolution in our concepts. This time we encounter very strong forces and seemingly unbreakable bonds acting among the individual quarks, leading us to question the possi-

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bility of ever detecting them in the laboratory. Are they objects whose existence can be inferred only from the properties of larger, complex structures, such as a proton, in which they are the constituents? Can we never study quarks and their structure by isolating them and probing them individually, as we do the electrons and nuclei of the atom, or the protons and neutrons that form complex nuclei? If this situation persists, physicists will be forced to question what is meant by the concept of elementary particles or basic building blocks. Unseeable and elusive elementary particles may even have to be replaced by an elementary "something else" that can be more directly related to laboratory measurements: elementary currents or symmetries, perhaps.

Why Quarks

The quark hypothesis was introduced in 1963 independently by Murray Gell-Mann and by George Zweig in an effort to summarize and systematize the great proliferation of nuclear particles that were being produced by accelerators on the high energy frontiers of the 1950s. Regularities were perceived in the masses of these particles as well as in the characteristics of their creation, their interactions, and their decay, for such was the fate of all but the proton itself in the subnuclear zoo that burgeoned to more than one hundred different inmates. Gell-Mann and Zweig showed that these regularities, as well as new ones found later, could be accounted for in terms of the simple motions and interactions of just three different kinds of quarks.

Because the quark hypothesis made correct predictions of new observations as well as provided a systematic organization of a large mass of data, and because it also brought simplicity plus a unifying harmony to our view of nature, the concept of quarks was a crucial step forward more than a decade ago, similar in many ways to the discovery of the nuclear atom by Ernest Rutherford in 1911. The periodic table of the elements, which had been developed around 1870 by Dmitri Mendeleev and shortly thereafter by Lothar Meyer, provided a systematic basis for modern chemistry. It also led to the discovery of several new elements whose existence was predicted on the basis of missing entries in the table. The first to be discovered was gallium in 1875. Furthermore, electrical properties of the atom were also being established around the turn of the century, but there was as yet no hypothesis or model of the constituents or of the structure of the atom itself. It was in the celebrated experiments of Rutherford that we first caught a glimpse of the picture of the atom that we are so familiar with today: a small, compact nucleus containing almost the entire mass of the atom, around which very light electrons circulate in orbits much in the same way as the planets orbit our sun.

Even before Rutherford's discovery the atom of the nineteenth and very early twentieth centuries was no longer viewed as the indivisible building block of all matter, as implied by its Greek name. However, its constituents and structure were still unknown prior to 1911. In particular it had been characterized as "plum pudding" by J. J. Thomson, who discovered the electron.

The complete confirmation of this picture of the nuclear atom was achieved in the decade following Rutherford's discovery, as physicists broke apart individual atoms and studied the fragments (i.e., electrons and ions). Rutherford

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was able to infer the existence of a very small, hard, and massive nucleus because the projectiles with which he bombarded thin foils of matter deflected through large angles, and some even bounded backward, much as does the cue ball in a break shot in pocket billiards. Knowing the nature of the electrical force between his projectiles and the target atom (in point of fact he assumed that the classical laws of electricity applied to atoms, but experimental confirmation of this came only later), he could quantitatively interpret his observed scattering pattern in terms of the atom's shape and structure. Later when these very same atomic constituents were also identified experimentally in the atomic debris as electrons and nuclei, the atomic model was confirmed. It was both understandable and successful. By 1922 the evidence had all been pieced together and the rules for atomic spectroscopy had been given in terms of the electronic structure of the atom. Quickly thereafter a grand synthesis was achieved with the development of the quantum theory, spearheaded by the works of W. Heisenberg, E. Schrödinger, M. Born, and others, starting in 1925. What emerged was a complete dynamical theory based on simple, elegant, and general physics laws; that is, mathematical equations plus unique, definite rules of physical interpretation including the understanding of the limitations on measurements made on the atomic scale.

Once the implications of a finite quantum of action for the observation and interpretation of atomic phenomena were fully acknowledged and understood, all the mysteries of atomic spectroscopy were stripped away. For atomic phenomena physicists could interpret known experimental results and predict new ones correctly from the same general laws and at the same time describe atomic behavior in a simple conceptual framework.

What we are trying to do now on the subnuclear frontier of elementary particle physics is simply to reproduce this triumph of fifty years ago. Whether or not we will succeed without a major conceptual upheaval is anybody's guess. The picture of electrons and nuclei as the basic constituents for describing all atomic properties and spectroscopic phenomena was built on the radically new conceptual foundations of the quantum theory that were developed in the 1920s. We are still applying these very same concepts today because experience has not yet forced us to abandon or modify them. No new principles were required as physics pushed ahead in the 1930s and 1940s to study what is going on within the nucleus. A rich variety of new phenomena was encountered in the study of nuclear structure on a scale of distances tens of thousands of times smaller than atomic dimensions. The nuclear forces and the spectra of excited nuclear states are very different from the atomic realm. There does not exist as yet a finished dynamical theory of nuclear phenomena at a fundamental level akin to our atomic theory. However, the interpretation of nuclear phenomena and the detection and identification of neutrons and protons as the constituents of nuclei has been accomplished wholly in terms of evolutionary advances within an existing conceptual framework.

There is no guarantee that this framework will still suffice as we push on another factor of a thousand and beyond to a higher scale of energies and a smaller scale of distances onto the quark frontier. But, as its frontiers advance, science can measure its need for new principles and concepts only by testing its existing ones. Therefore our identification of quarks as the basic building blocks

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of the elementary particle frontier invites comparison and contrast with what we mean when we identify electrons as constituents of the atom and when we identify neutrons and protons as constituents of the nucleus.

Nuclear Constituents

Let us first describe some of the new phenomena encountered when probing within the nucleus and recall how they were interpeted using known concepts. Following the discovery of the neutron in 1932 by James Chadwick, the nucleus was revealed to be a structure built of individual protons and neutrons bound to one another by a new force—a strong nuclear force that is still only partially understood today. This discovery was made originally by observing that individual protons and neutrons, as well as light nuclear structures such as alpha particles formed from them, emerged as the debris from nuclei that were given a hard smash by energetic particle beams. During the early 1930s nuclear physicists went so far as to project a simple world view with all matter, animate or inanimate, built of combinations of just three elementary building blocks, or fundamental constituents—electrons, protons, and neutrons.

Though appealing in its simplicity, this unifying view was nevertheless beset by fundamental problems. First of all it relied on the existence of a mysterious new and very strong nuclear force acting only over very short distances, less than the dimension of the nucleus itself, that binds the nucleus together. It is evident that electrical forces are not responsible for binding nuclei built of protons and neutrons: the protons all have the same electrical charge and therefore electrostatically repel, rather than attract, one another. Furthermore the neutron is electrically neutral and is therefore essentially immune to electrostatic forces. What, however, is the origin of the nuclear force? And what is its relation to the electrical forces responsible for atomic binding?

Another problem was the occurrence of beta nuclear radioactivity. These are nuclear tranisitions leading to the emission of electrons. However, the emerging electrons which form the nuclear radiation were observed to carry only a variable fraction but not all of the energy made available in the nuclear transition. It was soon realized that energy did not balance in the radioactive decay of nuclei to their observed decay products. Rather than abandon on he atomic scale the principle of energy conservation, which is so well verified and which is so important conceptually in macroscopic classical physics and chemistry, an additional unseen particle called the *neutrino* was postulated in 1930 by Wolfgang Pauli. It served as the agent to carry off the missing amounts of energy as well as to provide the momentum balance, as was confirmed by later precise measurements. The neutrino, which is a massless and electrically neutral particle (a little neutron and hence its name) fully earned its status as a respectable member of the particle family only after it was observed directly. "Observation" of a neutrino meant demonstration that it initiated observable reactions when impinging on matter as well as confirmation that, in the radioactive decay process, it carried away definite values of both energy and momentum characteristic of a single particle with zero mass. This story of the neutrino in the 1930s is an example of a fundamental principle being preserved—the

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principle of energy-momentum conservation—at the price of introducing a new particle, later confirmed by direct detection.

The simple picture of the early 1930s was further eroded by the introduction of a new force together with the new neutrino particle on which it acts. This is the so-called *weak force* responsible for the observed particle decay with beta emission processes of which the neutron decay is the most direct example. Physicists were now faced with very weak as well as very strong forces in addition to the electrical ones that bind the atoms. Subsequently, the remaining vestige of a simple unified view was completely buried in the debris of high energy nuclear collisions which were detected first in cosmic rays and shortly thereafter at the man-made large accelerators. Since the 1940s we have known that when protons and neutrons are hit very hard—as in the collisions of particle beams incident on hydrogen and deuterium targets at high energy accelerators—many fragments are produced. It is evident that both protons and neutrons are themselves also composite systems. Typically the proton changes its state of motion when bombarded by a high energy projectile such as another proton. Its internal excitations are generally very short lived and, as it returns to its normal state, it emits other short lived unstable particles. Some of these emerging fragments are heavier than the proton itself. For very energetic collisions, fragments of antimatter are also produced in abundance. These exotic forms of debris—very heavy particles and antimatter—make their appearance because, the more energy available, the greater the masses that can be created according to the famous Einstein relation $E=MC^2$. They also immensely complicate our quest for the subnuclear constituents of which the particles are built.

Where Are the Quarks?

The quarks were introduced to provide a simple conceptual basis for the observed patterns of subnuclear spectroscopy. Their role is similar to that of the electrons in atomic spectra and of the neutrons and protons in nuclear spectra. Although there is a fundamental dynamical theory, as expressed through Maxwell's equations, of the electromagnetic forces binding electrons in an atom, we do not have a fundamental theory of the strong forces binding either the protons and neutrons together or the quarks to one another. However, a highly successful predictive phenomenology, or scheme-of-things, has been created in the latter two cases for the nuclear and subnuclear spectroscopy. What is new, however, is that in contrast to both our atomic and nuclear experiences we do not detect the individual quarks isolated from one another. When we break apart, or *ionize*, an atom its electrons and its nucleus are clearly evident and are detected in the debris. The same is true when we identify individual protons and neutrons in the debris stripped away in nuclear collisons. How then do we account for our failure to detect quarks in the debris of a shattered proton or of any other subnuclear particle? Must we alter or abandon tested concepts, as Niels Bohr first proposed to abandon conservation of energy in the atomic decays of individual neutrons before Pauli's suggestion of the then undiscovered neutrino? Or is there a more elementary explanation within our established conceptual framework as to why quarks fail to show up in the subnuclear debris of energetic collisions?

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Massive Quarks

An elementary and conservative approach to the dilemma of the non-appearance of quarks is to suppose that individual quarks are so massive that they cannot be made by existing accelerators. According to this view the forces of attraction binding three quarks to one another to form a proton, for example, are enormously strong. In order to overcome these attractive bonds and free the quarks one has to add a very large amount of energy to the proton. After this very large amount of energy has been added, the mass of the system formed—in this example the three freed quarks—will be very much heavier than the initial proton as a result of the Einstein relation $E = MC^2$.

This view is an extrapolation to much stronger binding forces of known behavior in nuclear physics. Consider as the simplest nuclear example the nucleus of a deuterium atom, that is, the deuteron, which is composed of a proton and a neutron bound together by nuclear forces. It is a stable substance in nature. If undisturbed externally, the deuteron and its two mutually bound constituents live forever. However, if the deuteron is smashed apart into an isolated proton and neutron we observe that the neutron decays into a proton with the emission of beta radioactivity in about twenty minutes. Evidently the free neutron is sufficiently more massive that it becomes unstable and exhibits a very different behavior than one bound in the deuteron. In fact, however, the binding energy between the proton and the neutron is only a fraction of a percent of the energy of a free proton and neutron, and barely holds them together. In contrast, the subnuclear interaction energies are a factor of a thousand or more greater than those in the deuteron and other typical nuclear systems. Hence their bindings are also comparably stronger, and there is a possibility that the proton itself has but a tiny fraction of the mass that its individual quark constituents would have when they are isolated from one another and free. According to this view there is nothing fundamentally new in the picture of protons made of quark constituents; we just need higher energy accelerators before producing single quarks.

Future experiments may support this picture, but for a number of reasons, some of which are experimental and some theoretical, many physicists are skeptical. For one thing extensive searches for evidence of quarks surviving the initial seconds of the existence of the universe or arriving with the cosmic radiation incident throughout the earth's history have all failed. It is becoming increasingly difficult to explain their nonobservation if they indeed exist. On the other hand, one may assume that very massive quarks are unstable and die so rapidly—in less than trillionths of a second, a typical time unit on the subnuclear scale—that we cannot recognize them as we do the very long-lived neutron constituent of the deuteron with its twenty-minute lifetime. This is again a possibility that cannot be ruled out at present, although it leads to difficulties in the detailed interpretation of existing data, and it lacks any supportive evidence at this time.

One of the most difficult general problems with the very massive quark models is simply the fact that the quarks were introduced originally to provide a simple understanding of subnuclear spectroscopy in terms of relatively light constituents moving approximately as independent particles. The intuitive sim-

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plicity of this picture is lost if we must resort to a model with very massive quarks bound together very strongly and moving in tightly bound systems such as the proton. Just as a proton is very much lighter than its constituent quarks, we would have to face a picture of the universe which had lost most of its material mass when matter formed!

Where do we go from here? Nature is telling us on one hand that quarks behave as light, independently moving constituents in a proton. This aspect of quarks is revealed by the general pattern of quark spectroscopy as well as by detailed interpretation of experiments using very high energy particle beams produced at the accelerators. On the other hand, nature has at the same time revealed quarks to be strongly and tightly, if not permanently, imprisoned together in the subnuclear particles. In fact, essentially all of the observed subnuclear particles, both the stable and unstable ones, can be classified into one family, the *hadrons*, which has two major branches. One branch, which includes the proton and all of its many dozens of cousins, is built of three quarks bound to one another in different states of motion. If we assume that there are just three different kinds of quarks, all members of this branch, known as the *baryons*, find a natural classification. The second, or *meson* branch, is built of one quark and one antiquark bound together. The existence of antiquarks along with the quarks themselves is a consequence of Einstein's theory of special relativity and the principles of quantum mechanics, according to which there must be an antiparticle for each kind of particle in nature.

Peculiarly absent from nature are structures built of two quarks, four quarks, or of two quarks and one antiquark, for example. The basic quark combination is always observed to be one quark with one antiquark, or three quarks or three antiquarks together; and no other combination occurs. If one of these baryon or meson particles is shattered in a nuclear collision new particles are formed, each of which is also itself characterized by the same quark content of three quarks, three antiquarks, or one quark and one antiquark. But never have isolated individual quarks been detected!

Analogy to a Bar Magnet

It has been frequently noted that this behavior is analogous to that of magnetic material with which man has been familiar since the early Greek and Roman times when the discovery of naturally occurring magnetic substances such as lodestone was historically first recorded. A magnet always has both a north pole and a south pole. When broken in two, a bar magnet becomes not isolated north and south poles separated from one another, but two magnets each with its own north and south poles. This is very similar to what was being described above: when a meson made of a quark and an antiquark is smashed apart, single quark and antiquark fragments do not emerge. The debris of the shattered particle consists of more mesons, each with its own quark and antiquark; or of more baryons and antibaryons, each made of three quarks and three antiquarks, respectively. From the occurrence of baryons it is evident that the analogy of quarks with magnetic poles is not a literal one. There is no magnetic correspondence to the baryon as there is to the meson. The fact that, in addition to the mesons, baryons made of three quarks exist whereas there are no

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"magnets" with three north poles is a result of the underlying formal mathematical structure of the quark theory. Nevertheless, the physical analogy of quarks with magnetism is sufficiently close and accurate to be a useful guide.

Our curiosity and present plight with quarks is not very different from that of an inquisitive mariner at sea about ten centuries or so ago. In a moment of calm on a passage he might have viewed a (spare) compass needle, which is just a simple bar magnet, with idle bafflement or scientific curiosity and tried to break it apart in order to separate the north pole from the south pole. But to no avail, for with each breaking of the compass needle he ended up with an additional one having both a north pole and a south pole. The understanding of this impossibility to isolate single magnetic poles came only many centuries later when, in 1820, the French physicist André Ampère first explained magnetism in terms of electrical currents. In fact a fundamental theory of magnetism at the atomic level in terms of the currents of circulating and spinning electrons was achieved only in this century on the basis of the modern quantum theory. When an electrically charged particle circulates it forms a current loop, thereby producing a magnetic field in a pattern similar to that of a compass needle or a bar magnet with both a north pole and a south pole. Many atoms then make a big bar magnet, but never a single pole.

The experimental search for magnetic monopoles continues, as it must also for quarks, for we can never know for certain that they do not occur under exotic or very rare circumstances. However, for all known matter the unbreakable bond between magnetic north and south poles is understood at a fundamental atomic level. Just as a meson built of a quark and an antiquark is not a fundamental particle, a magnet is not the "elementary unit" of magnetism. The elementary unit of magnetism is also not a single north or south pole. However, if we view the quark itself as a fundamental particle we are breaking this analogy with magnetism. On the other hand, if we wish to pursue the analogy of quarks with magnetism, a new "elementary structure," analogous to the atomic current, must be introduced.

A New Elementary Structure?

In either event we face a new point of departure, leading either to a new "elementary structure" or to quarks mysteriously tied together as if by an unbreakable string. If the former is the case, we have neither seen nor theorized creatively as to what these new "elementary structures" are. However, we can hope that, given the expensive and large facilities of high energy physics that are required in order to probe with increasing sensitivity on smaller and smaller scales of distance, it will take less than the nineteen centuries required to advance from lodestone to amperian currents. On the other hand, if the quarks are indeed the elementary particle, then a new kind of dynamics is required, different from that with which we are familiar on the atomic or nuclear scale. In atoms the electrical forces decrease with the inverse square of their separation as the distances between the charged electrons and nuclear protons increase; these forces increase with decreasing separation. For the quark dynamics we seem to find an opposite behavior: when the separation between quarks is small, as deep within a proton or meson, their interaction is relatively weak. However, it

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grows in strength when they are no longer very close together and remains strong even for large distances between the quarks. This behavior is frequently called *quark confinement* or *quark trapping*. It takes more and more energy to pull a quark and an antiquark further apart against the attraction of this force. Eventually the energy invested increases to the point that it equals the amount required to create a quark-antiquark pair. When those materialize we have the analogue of splitting the compass needle in two; that is, we have two mesons, each built of an original quark and its materialized antiparticle.

There is optimism that the general structure of dynamical theories exhibiting such properties has been specified. The technical problems of solving these so-called asymptotically free gauge theories have not been all worked out, but special calculations have led to encouraging results in some cases. Whether the observed subnuclear particles are built out of elementary currents analogous to the amperian currents underlying magnetism in a future theory of the submicroscopic world, or whether they are built out of elementary quarks permanently tied together as if by strings so that we cannot isolate them in the laboratory, our concept of the elementary particle will be greatly changed. In any event, unless we actually detect the individual constituents, modern physics will find itself closer to the biblical description found in Chapter 11, Verse 3, of the Epistle of St. Paul to the Hebrews: "Through faith we understand that the worlds were framed by the word of God, so that things which are seen were not made of things which do appear."[6]

Werner Heisenberg approached this issue from a different viewpoint in a lecture on the nature of elementary particles delivered in 1975 shortly before his death. He raised the possibility that the root of our present dilemma is that we are asking the wrong question in particle physics when we ask what a proton "consists of":

> I will now discuss that development of theoretical particle physics that, I believe, begins with the wrong questions. First of all there is the thesis that the observed particles such as the proton . . . consist of smaller particles: quarks . . . or whatever else, none of which have been observed. Apparently here the question was asked: What does a proton consist of? But the questioners appear to have forgotten that the phrase "consist of" has a tolerably clear meaning only if the particle can be divided into pieces with a small amount of energy, much smaller than the rest mass of the particle itself.[7]

Heisenberg is referring here to the fact that very high energies are required to overcome the strong binding forces and shatter a proton into its various forms of debris, or pieces, many of which may be as massive or more massive than the proton itself. These energies are hundreds to thousands of times greater than those required to break apart a nucleus and billions of times greater than needed to break up the atom. We are no longer dealing with energies that are but small fractions of the rest masses of the particles themselves. We can no longer rely solely on nonrelativistic ideas of slowly moving particles and weak forces. Rather we must contend with the special theory of relativity because in the presence of strong binding forces and high energies the distinction between energy and mass can no longer be clearly made. Both matter and antimatter are present and the energy can transform back and forth between the various possible forms into which it can materialize.

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We must also take into account another aspect of the theory of relativity, and that is the fact that the force between neighboring particles such as between two quarks does not act instantaneously but is transmitted no faster than the speed of light. If we are dealing with weakly bound systems with slowly moving constituents we can neglect the fact that the speed of light, although very great, is finite; or we can take it into account by suitable simplifying approximations. Within baryons and mesons this is no longer possible and the dynamics of how energy and momentum are exchanged among interacting quarks and bind them together is central to the description of the structure of subnuclear particles. We have earlier referred to unbreakable strings that tie the quarks to one another. These strings may be fundamental new dynamical objects, or, viewed more conservatively, no more than an approximate description of the effects of the elementary quanta known as *gluons* that carry the discrete units of energy and momentum between quarks and antiquarks, "gluing" them into mesons and baryons. On the subnuclear level the gluons are the analogues of the photons or quanta of the electromagnetic field for atomic processes. A complete dynamical description of the structure of the proton necessarily prominently involves the gluons, or strings, as well as the quarks themselves. Moreover, as the quarks move around, the pattern and numbers of gluons binding them to one another also change. So do the forms of matter and antimatter into which the gluons can materialize. Hence a complete picture of what the proton consists of is less than clear, as Heisenberg suggested. Most physicists agree, however, that the simple quark picture is a very useful, if not a literal, picture of the structure of hadrons.

How Many Quarks?

The motivation for introducing quarks in the first place was to bring unity out of diversity—to attempt to explain the construction of a vast number of particles and their properties using only a few building blocks. At the atomic level this was achieved by introducing the electron and building up all the known elements in terms of different numbers of identical electrons in different atomic orbits. At the nuclear level a basic classification of all nuclei was achieved in terms of different combinations of just two kinds of particles—the proton and the neutron. We of course also want to know how many different kinds of quarks there are as the subnuclear building blocks. In the original proposals of Gell-Mann and of Zweig there were three different kinds (denoted by u, d, and s for up, down, and sideways, or "strange," with the physicists' usual penchant for simple and wry vocabulary). These three, together with their inevitable antiquarks, were all that was required to explain all observed nuclear particles.

A novel feature of the quarks as originally proposed is that they are assigned electric charges that are fractions of the electron charge. All observed particles in nature have either zero electric charge or an integer number of units of the electron charge. As fractionally charged particles with either one-third of the electron's charge (the d and s quarks) or two-thirds and the opposite sign of the electron's charge (the u quark) quarks were a new invention.

The two quarks with the same one-third unit of charge differ in another physical property, or in the language of physics, in one of their *quantum numbers*. Physicists identify and describe subatomic particles by assigning them

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quantum numbers; each number designates a property that is conserved, or left unchanged, when particles interact. Some quantum numbers, such as electric charge and spin angular momentum, refer to physical, measurable attributes of the particle. Others are more abstract; they denote family resemblances among particles and provide a valuable bookkeeping system for classifying particles and their interactions in algebraic form.

Some quantum numbers—such as electric charge—are always conserved. Certain others are only "approximately conserved"—that is, for most interactions they are conserved but in some cases, the rare ones, they are not. On the way to a fundamental dynamical theory the search for conserved quantum numbers has been a very productive and valuable one for physicists. For each quantum number there exists a corresponding symmetry of the underlying dynamical laws. With each discovery of a quantum number, or conservation law, the possible forms of the underlying laws of nature—the "holy grail" of natural science—are further delimited and the search more narrowly focused.

The great excitement and frenetic activity in elementary particle physics during the past two years, that I mentioned at the outset, has been caused by the experimental discovery of an additional new species of quark: a fourth quark carrying a new quantum number called *charm*. Like the three quarks before it, the new quark with charm was not directly detected by itself. What was actually observed in the laboratory was a new family of particles with extraordinarily surprising properties: they were very heavy, more than three times as massive as the proton, but still they lived for a very long time, about ten thousand times too long on the subnuclear time scale. In order to accommodate the new particles in a Mendeleev or Gell-Mann/Zweig table for quarks, a new species, the fourth quark, was required.

But it is not quite that simple. As the quark hypothesis was applied in detail during the past decade it became clear that the full body of data to which it was applicable could be accommodated and interpreted in terms of known fundamental particle properties only if each of the three, and now four, different quarks was endowed with yet another quantum number that could assume just three possible values. This new property was dubbed colloquially *color*, though it has nothing to do with the usual meaning of color. In effect each of the four different kinds of quarks was tripled so that with this extension of the quark family there are now altogether twelve quarks as basic building blocks (along with their inevitable antiquarks). In addition more may be required if, as the experiments reach to higher energies, new genera of particles are encountered. To some physicists there are already compelling suggestions based on the detailed analysis of recent experiments that no fewer than six different kinds of quarks, each occurring in three colors for a grand total of eighteen, are required. One wonders whether the family of quarks has not already grown so large that there are too many for them to be legitimate candidates for "fundamental particles"! Perhaps a smaller family of basic currents and a simpler scheme of symmetries will mark the next major advance analogous to the amperian currents underlying magnetism. Whether or not the quark is the "end of the line" as the Greek "atom," the scientist can be sure of one thing: nature's imagination has always proved richer than man's vision of what lies ahead beyond the next frontier of scientific inquiry.

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Unity of Forces

It is the fundamental faith of a scientist that simple general laws exist in terms of which the rich diversity of nature can be explained. As noted earlier in quoting Heisenberg, we may question whether the search for basic building blocks or elementary constituents of the subnuclear world has been correctly posed. But there is no doubt about the ultimate goal of a unified theory which reduces the broad variety of experience to simple and general laws. To achieve this goal we must know not only the basic constituents or currents that interact with one another; we must also know the nature and variety of the quanta transmitting the forces, as well as the relation between quanta and the currents or constituents that are their sources.

At present there are four distinctly different kinds of forces that are believed to account for all observed interactions of matter: gravitation, electromagnetism, the strong force, and the weak force. In the everyday world gravitation is the most obvious of the four forces; it influences all matter and acts over very large distances. For the infinitesimal masses involved in subatomic events, however, its effects are vanishingly small and can be ignored. Its quantum is called the *graviton*. The electromagnetic force is also felt over very large distances, but it acts only on matter that carries an electric charge or current. The *photon* is the quantum, or carrier, of the electromagnetic force, and when two particles interact electromagnetically, they can be considered to exchange a photon or photons.

Following the development and confirmation of his general theory of gravitational phenomena, Einstein and his colleagues spent three decades in a futile effort to construct a unified theory including the effects of both gravitation and electromagnetism in a single general force law describing all phenomena of classical physics on a macroscopic level. In the realm of atomic and subnuclear phenomena we may ignore the very weak gravitational effects, but we face the very great challenge of unifying electromagnetic forces with the weak and the strong ones.

The strength referred to in the names of the strong and the weak interactions is related to the rate at which the interactions take place. The strong force is short ranged; that is, its influence extends only over a distance comparable to the sizes of the subnuclear baryons and mesons. When two particles that interact by means of the strong forces approach within this distance there is a high probability that they will deflect one another or that they will produce other baryons or mesons. Compared with the strong force the weak one, which is responsible for the neutron decay, is very feeble indeed. It is weaker by a factor of roughly ten trillion for low energy collisions. It is even shorter in range by a factor of a hundred or so than the strong forces, and it has the unique property of growing in strength as the collision energy of the interacting particles increases.

Apparently the weak, electromagnetic, and strong forces have little in common, and the effort to unify them would seem to be hopeless, or at best discouraging, were it not for the guidance available from symmetry principles and conservation laws.

The first crucial step in the unification of the weak and the electromagnetic

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forces dates back to 1967 and is largely the work of Steven Weinberg and Abdus Salam. If the weak processes, such as neutron decay with beta emission, are governed by a theory of the weak forces that is constructed on the same symmetry principles as the theory of electromagnetism, we would expect to find common features of the weak and electromagnetic interactions. For example, for the weak processes massless quanta should be radiated and absorbed just like photons, and the weak forces should extend over very long ranges. This is very far from the observed case, however. There are no massless quanta for the weak forces, and the extreme difference in the apparent character of the short-range weak forces from the very much stronger and long-range electromagnetic forces defied attempts at unification for many years prior to 1967. It was realized then that the fundamental equations of the weak and electromagnetic interactions could in fact be unified under a common symmetry principle, but—and this was the key new idea—the symmetry is partially broken in a well-defined and simple way for weak processes, though not for the electromagnetic ones, which honor it. As a result of this symmetry-breaking, the quanta of the weak interactions are predicted to acquire a mass approximately forty or more times heavier than that of a proton. It is this large mass that is responsible for the very different character of the weak and electromagnetic processes as studied with present accelerators.

However, when we probe the forces at very small distances the unification schemes predict that the two interactions become very similar in character. In order to experimentally study such small distances, which must be well within the short range of the weak forces themselves or less than one hundredth the size of the proton itself, interaction energies are required beyond those that can be achieved with existing accelerators. This condition defines the new high energy frontier of elementary particle physics. "High energy" in this context means energies that are large when compared with the mass energy of the weak interaction quantum, or energies hundreds of times larger than the mass energy of the proton itself. On such a high energy and small distance scale the mass of the weak interaction quantum can be neglected and the common features of the weak and electromagnetic forces will be revealed if these very attractive ideas of an underlying symmetry principle are correct.

There are already experimental results in accord with some of the predictions of this approach to a unified theory of weak and electromagnetic processes. In particular, new types of neutrino-scattering processes can be, and were, predicted. These have since been observed and have a strength compatible with the theoretical framework of the unified theories. The predicted properties of the quanta of the weak interaction processes include their large masses. Although the thresholds for producing them lie beyond the ranges of energies accessible to the existing accelerators, they are within the range of the next generation of machines now being planned and designed. Physicists are confident that these quanta will be found!

Strong Forces

That leaves the strong processes—those involving the quarks and gluons—still to be explained. Detailed progress here is more difficult because the mathe-

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matics of dealing with very strong forces is technically much more formidable. Even if our equations are correct we have yet to construct their accurate solutions! Yet there is optimism that, by once again modeling our ideas on the symmetries found in the theories of electromagnetic (and now also of the weak) processes, we may be making progress. In this case gluons are the quanta and also acquire large masses because of a broken symmetry. Quarks carry the color quantum number, as noted earlier, and interact with the gluons in much the same manner that electrons interact with photons, the quanta of the electromagnetic field. Color plays an analogous role to electric charge for the electron: only the charged particles interact with the electric forces, and similarly only quarks with the quantum number color, and baryons and mesons built out of quarks, react to the strong forces. Electrons and neutrinos do not carry color and hence do not react to strong forces.

With this approach the color quantum number acquires a fundamental importance as the "charge" of the strong forces. The absence of isolated quarks is then equivalent to the absence of any states of color in nature. According to the mathematical formalism associated with the idea of color, the only structures that can "hide their color" are those formed of the combinations of a quark and antiquark, or of three quarks. These are the color analogues of electrically neutral systems, and they are the only ones to occur in nature. On this basis we can understand why all observed mesons and baryons have this quark structure and none other: they are neutral in color and hence not confined.

Analogies often prove very useful to guide scientific inquiry. If taken too literally and followed uncritically they also can lead one astray. In no way can they substitute for a complete dynamical theory with predictive power. Whether the analogy of quarks with magnetic poles as described earlier, or that of color with electric charge as given above, remains very useful for very long is uncertain. However, the very possibility that, guided by symmetry principles, we are on the right track toward a unified theory of nature's laws has had a very exhilarating effect on elementary particle physics. Predictions of allowed, inhibited, and forbidden processes have enjoyed experimental successes. However, grave hazards remain because the theoretical structure is still in the process of being constructed and is necessarily largely untested. At least we have, for the first time, simple, unified, and even aesthetic principles as guides.

This theory that is built in terms of quarks and gluons that are permanently confined, and in terms of the "hidden" quantum number color that, unlike the electric charge and other such quantum numbers, also can never be seen, carries with it the ghost of the aether. In the late nineteenth century there was prodigious scientific activity devoted to endowing the aether, or the medium thought to transmit light and electric signals, with a physical reality: it drifted in space, it dragged, it compressed, etc. At the same time, however, one never observed the aether, and there were increasingly intricate and artificial excuses for hiding evidence of it from experiment. The web of intrigue surrounding the invisible aether was cleared away by Einstein in 1905 with his theory of special relativity. Some of today's elementary particle physics concepts, such as color, not to mention the quarks themselves, remain hidden from direct experimental observation. It is not beyond the realm of possibility that they may also suffer the fate of the aether, though few today find it possible to conceive of establish-

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ing an underlying unity on the subnuclear frontier without the quarks. And what will happen to this theoretical edifice being constructed if isolated quarks are in fact observed in the laboratory?

Conclusion

As its frontiers advance in the exploration of ever smaller distances and systems, the concepts, if not the vocabulary, of elementary particle physics grow more and more abstract. Still there persist strong intellectual bonds with other fields of natural science as a vital source of stimulation. For example, what we learn on the subnuclear frontiers is already shaping the astrophysicists' views of what took place in the first seconds of the "big bang" at the creation of our universe. It contributes to our understanding of the continuing evolution at the far reaches of outer space as new objects of highly concentrated energy are discovered and studied. Also the richly developed theory of critical phenomena and phase transitions in the field of many-body physics concerned with the behavior of macroscopic solids, liquids, and gases has provided seminal ideas for the study of quark confinement.

In order to further advance the subnuclear frontiers, higher energies and hence bigger accelerators and detection devices are also required. There are but few centers in the world where the frontiers can still be experimentally probed with the very sophisticated and complex equipment on which we must rely for the data that are the lifeblood of physics—and of all natural science. As the number of such facilities decreases and the complexity of performing the experiments themselves increases, large research teams and collaboration on an international scale become the norm. The field of elementary particle physics is unavoidably and unquestionably "big science."

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The Limits of Space and Time: Exploding Black Holes and the Origin of the Universe

Introduction

THIS ARTICLE IS WRITTEN in celebration of two great contemporary discoveries, one observational and one theoretical, which have brought us close to the limits of space and time. The observational discovery was made by accident and the theoretical ones were strokes of genius. Nevertheless I believe that we can learn from them how to probe even deeper into Nature's secrets in the coming years. What is needed above all is sophistication of technique allied to a profound understanding of the underlying physics at an elementary level, together with a determined attempt to unify the various branches of the subject. I hope that the truth of this approach will become evident as I describe the two discoveries and their interrelations.

1. Relict Radiation from the Hot Big Bang Origin of the Universe

In 1965 two radioastronomers of the Bell Telephone Laboratories discovered quite unexpectedly that at their operating wavelength of 7 centimeters there appeared to be an excess flux of radio noise arriving at the Earth. This flux was about one hundred times greater than would be expected from combining together the radiation from all previously known radio sources. The astronomers, A. A. Penzias and R. W. Wilson, carefully considered various possible sources of experimental error, but finally concluded that the excess was real, and of extraterrestrial origin.[1]

Many interpretations of this excess radiation have been proposed in the years following its discovery, but the most widely accepted is the very first one to be considered. This was the suggestion by R. H. Dicke, P. J. E. Peebles, P. G. Roll, and D. T. Wilkinson[2] of Princeton University (made in ignorance of a previous prediction by G. Gamow[3]) that one is here dealing with radiation left over from the hot big bang origin of the expanding Universe. Near the time of the big bang the density of the Universe would have been very much greater than it is today (because of the subsequent expansion), and at such large densities any radiation present would have attained thermal equilibrium with matter. Accordingly it would have achieved the so-called Planck spectrum which

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characterises black body radiation. It would then retain this spectrum during the subsequent expansion but with a steadily decreasing temperature. By contrast, if there had been no big bang and the Universe were in a steady state, with the continual creation of new matter maintaining a constant density at all times despite the expansion, it would be difficult to account for the presence in the Universe of thermal radiation. To put it at its starkest, if such radiation were found, it would show directly that the Universe as a whole was once about 10^{15} more dense than it is today, because that would have been the lowest density at which the radiation could have been thermalised in the time available.

The importance of establishing the spectrum of the radiation is now clear, but we must remember that Penzias and Wilson had measured its intensity at one wavelength only. If the radiation were indeed thermal, then this intensity would imply that its present temperature would be about 3 degrees above absolute zero. Subsequent measurements at other wavelengths by a variety of experimenters have gone a long way towards establishing a thermal spectrum for the radiation, but we still await definitive results from satellite observations made above the Earth's atmosphere, where the observations at the most critical wavelengths have to be made. Nevertheless most astronomers agree that the case for the thermal character of the radiation is now very strong, and for the purpose of this article I shall assume that it has been established.

In this spirit we can then say that by observing the black body radio background we are in effect directly observing processes which occurred in a Universe vastly different from its state today, and at a time at most about 300 years after the big bang (whereas the present time is about 10 billion years later). However, we can go even further back if we accept the view of most astronomers that nearly all the helium we observe today was formed close to the hot big bang, at a time when the radiation temperature was more like 1 billion degrees than 3 degrees. Peebles showed in 1966 that this was a high enough temperature to ensure that nuclear reactions would have led to the formation of helium from hydrogen at the rate required to account for its presently observed abundance. If this argument is correct, it would mean that we can see today the outcome of processes which occurred one hundred seconds after the big bang, when the density throughout the Universe was more than 10^{25} greater than it is today.

The existence of the 3° K radiation thus has remarkable implications. But nearly as remarkable are the implications of the further observation (made in 1967 by E. K. Conklin and R. N. Bracewell,[4] and by R. B. Partridge and D. T. Wilkinson[5]) that its temperature in different directions in the sky does not vary by as much as one part in a thousand. This is by far the most accurate observation ever made in cosmology. It tells us immediately that the Universe expands at the same rate in all directions to the same precision, and that on a large scale the distribution of matter in the Universe is the same in all directions (isotropy) and at all places at one cosmic epoch (homogeneity).[6, 7] This means that the grand design of the Universe is remarkably uniform. It thus presents us with the ultimate Copernican principle: mankind inhabits a typical planet which is bound to a typical star contained in a typical galaxy belonging to a typical cluster of galaxies. . . .

We can summarize the discussion so far by saying that observations of the 3° K background and the abundance of helium made over the last twelve years have shown that (a) the steady state theory of the Universe is wrong; (b) the

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Universe was once 10^{25} denser than it is today; (c) we can study the results of processes occurring 100 seconds after the big bang; (d) on a large scale the Universe is remarkably homogeneous and isotropic.

This represents gratifying contemporary progress in our understanding of the Universe. The most important problems that remain are (1) What is the nature of the big bang? (2) What is the origin of the 3° K radiation field? (3) Why is the large-scale Universe so symmetric? Lying behind these problems, of course, is the deeper one: Can questions of this sort be answered at all by the methods of science? Let us now see how far we can go in trying to answer them.

Was the Big Bang a Singularity?

The year 1965 saw the publication not only of the momentous paper by Penzias and Wilson but also of one by R. Penrose entitled "Gravitational Collapse and Space-Time Singularities."[8] In that paper Penrose was concerned with collapsing stars rather than with the expanding Universe. I want to discuss the stellar case in the next section but for the moment I shall concentrate on the application of Penrose's ideas to the whole Universe by S. W. Hawking and G. F. R. Ellis in 1968,[9] following on Hawking's work over the years 1966–1968.

To introduce the idea of a singularity let us consider a simple model of the Universe consisting of a uniform fluid which is expanding uniformly and which is subject to the Newtonian law of gravitation. Such a model can be regarded as a rough representation of the actual Universe. Its evolution in time is easily calculated, and one finds that about 10 billion years ago all the "particles" of the fluid would have been on top of one another, leading to an infinite density of matter. This infinity is called by the mathematicians a *singularity*. Of course, one would not expect the familiar laws of physics to hold good under such extreme conditions. What is not clear, however, is whether new laws need to be invoked or whether deviations from uniformity would have prevented an extreme density from ever being set up. The effect of such deviations is most easily seen if we mentally reverse the sense of time and picture the Universe as collapsing. We might then imagine either that pressure gradients could build up, leading to a "bounce" at some large but non-extreme density, or that irregular motions of the atoms in the Universe would ensure that they did not end up in one place at one time, but interpenetrated one another so that a singularity would be avoided.

What is the situation if we replace Newton's law of gravity by Einstein's, that is, by the general theory of relativity? For a highly symmetrical model of the Universe the answer is relatively easy to obtain, and has been known since 1922. In that year a Russian meteorologist, A. Friedmann,[10] found an exact solution of Einstein's equations which represented an expanding Universe (this was seven years before Hubble's[11] first observational pronouncement on the subject). As in the Newtonian theory the model has an infinite density at the origin of time. However, the singularity is now much worse. In Newtonian theory space and time are absolute, that is, are unaffected by the presence and motion of matter. Thus the Newtonian singularity relates to matter only, the structure of space and time remaining quite normal. In Einstein's theory the geometry of space and time becomes dynamical and coupled to matter. As a result the singularity is also a singularity of space-time.

This is, in fact, a reductio ad absurdum, because one of the axioms on which

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general relativity is built is that space-time is non-singular. Thus this theory contains within itself the seeds of its own destruction. Nor can we now avoid the issue, as was thought possible before 1965, by appealing to irregularities as a means of avoiding the singularity. For the "singularity theorems" of Penrose, Hawking, and others show that, on the basis of relatively weak assumptions, even an irregular Universe starts off with a singularity. Roughly speaking we can attribute this change from Newtonian theory to the greater self-gravitation which matter possesses according to general relativity.

This result is of such capital importance for our understanding of the Universe that I would like to indicate how weak the assumptions are on which one must rely. In some versions of the singularity theorem the assumptions, though weak, are impossible to verify observationally. But there is one version, contributed by Hawking and Ellis,[9] in which the main assumption can indeed be verified. Curiously enough, this depends on the properties of the 3° K cosmic background radiation, the first observation of which was made in the same year as the first singularity theorem was discovered.

In fact the 3° K radiation field enters the argument in two different ways. In the first place, its degree of isotropy gives us a numerical measure of how closely the actual Universe conforms to the highly symmetrical models which are known to contain a singularity. Secondly, the attractive gravitational effects of the 3° K radiation field itself contribute significantly to the tendency of gravitation to produce a singularity. When these two considerations are brought together one finds that, according to general relativity, the actual Universe must have started out with a singularity.

With this conclusion we have arrived at the crisis which presently confronts those who study the early stages of the Universe. It is too early yet to tell how this crisis is going to be resolved. On the most conservative view it would be sufficient to introduce into the argument the quantum theory of fields, that is, the branch of physics used for the study of elementary particles. There is little doubt that the processes occurring in the Universe in the first tiny fraction of a second after the big bang would need quantum theory for their proper discussion. Whether this would suffice to remove the initial singularity is unclear. It may be necessary in addition to introduce totally new laws of physics. We are here at the edge of the unknown but not, I hope, of the unknowable.

Black Holes

Penrose's original singularity theorem was devised for a collapsing star rather than an expanding Universe. It is therefore directly applicable in astronomy, because there must be upwards of a billion stars in our Galaxy which have burnt up their internal energy sources, and so can no longer produce the heat needed to oppose the inward force of gravity. These are stars of more than a few solar masses, which evolve much more rapidly than does a star like the sun. Moreover, it is just these more massive stars whose collapse cannot be halted at an intermediate stage (that is, as a white dwarf or a neutron star). Instead the collapse must continue until a singularity is reached.

It would be wonderful to observe such a singularity, and so to obtain direct evidence of the undoubtedly bizarre phenomena that occur near a singularity.

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Unfortunately in all those cases amenable to calculation using general relativity one finds that the singularity cannot be seen by a distant observer. What happens is that outgoing light rays are dragged back by gravity so forcefully that if they were to start out within a few kilometers of the singularity they would end up in the singularity itself. Accordingly, however bizarre the phenomena that may occur near the singularity, an external physicist would remain oblivious of them. In other words, a black hole would have been formed.

What we would dearly like to know is whether a black hole always forms when a star collapses catastrophically, or whether the outcome is ever a naked singularity. Penrose has coined the phrase *cosmic censorship hypothesis* for the statement that an indestructible horizon always does form around a singularity resulting from the collapse of a star. Its proof or disproof from general relativity remains an outstanding challenge to mathematicians.

It is true that the introduction of quantum effects or of totally new physical laws may one day eliminate the singularities. Nevertheless such changes in basic theory would be unlikely to have an appreciable effect on a collapsing star until it had reached a very high density indeed. The question whether such an extreme regime could be available for study from outside, or would always be concealed behind a horizon, thus remains of crucial importance.

It would also be important to observe a black hole, both as a check on the correctness of general relativity, and for its intrinsic physical interest. Curiously enough the best candidate is an object first detected in 1965, the annus mirabilis when Penzias and Wilson published their first measurement of the 3° K background and Penrose the first singularity theorem. The object concerned is an X-ray source, Cygnus X–1, which was detected by S. Bowyer, E. T. Byram, T. A. Chubb, and H. Friedman[12] in a rocket flight above the Earth's atmosphere. It was not realized at the time what a remarkable object it probably was. It needed further X-ray observations, by the Uhuru satellite, followed by radio and optical observations, to show that the X-ray source is revolving around an optical star. This discovery enabled B. L. Webster and P. Murdin,[13] and independently C. T. Bolton,[14] to estimate in 1972 that the mass of the X-ray source exceeded 3 solar masses.

This was a critically important estimate because once it was realized that the X-ray source had a stellar companion, it became plausible to suppose that the X-rays were emitted by gas captured from the companion's atmosphere and heated as it fell towards the X-ray source. The detailed data then showed that the X-ray source could not be as large as an ordinary star, but had to be compact and so could only be a white dwarf, neutron star, or black hole. The mass estimate then becomes decisive, because neither a white dwarf nor a neutron star can have more than 3 solar masses. Accordingly both Webster and Murdin, and Bolton, proposed in 1972 that Cygnus X–1 contains a black hole.

This proposal has led to many subsequent studies of Cygnus X–1, both observational and theoretical. The case for a black hole is now quite strong, but it remains indirect and by no means certain. What we need is an observation which makes sense only if a black hole is present. It has recently been suggested by R. F. Stark and P. A. Connors[15] that one such observation may be the linear polarization of the X-rays, or rather the way the polarization depends on the X-ray energy. Future satellite observations should be able to test this idea. It would be

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very exciting if this test were successful, because in addition to confirming the presence of a black hole, it would provide us for the first time with a general relativity effect that is large (for instance, a rotation of the polarization direction by 90°) instead of the usually minute quantity which can barely be detected.

2. *Black Hole Explosions*

We now come to the second stroke of genius which we are celebrating in this article, namely, Hawking's discovery in 1974 that a black hole formed from stellar collapse is not an immutable object if left to itself.[16] Instead it evaporates away as though it were a hot body, slowly at first but with gathering speed, until it ultimately dissipates itself in a violent explosion in which a variety of elementary particles and hard radiation (high energy gamma rays) is produced. This astonishing idea is already widely accepted, although certain detailed aspects remain controversial.

Hawking's starting point was the growing realization in the years 1970 to 1973 that, when quantum effects are taken into account, black holes obey the laws of thermodynamics. These ideas were codified in 1973 on the one hand by J. D. Bekenstein[17] and on the other jointly by J. M. Bardeen, B. Carter, and Hawking himself.[18] This work showed that one could attribute a temperature and an entropy to a stationary black hole in such a way that the usual laws of thermodynamics would be satisfied if the dynamics of black holes are governed by Einstein's field equations of general relativity. The quantum nature of this result was evident from the appearance of Planck's constant in the expression for black hole temperature and entropy. If one takes the classical limit by setting this constant equal to zero, the temperature of any black hole becomes zero and its entropy infinite. In the quantum case the temperature of a non-rotating black hole is inversely proportional to its mass, and the entropy is directly proportional to the surface area of the event horizon. Similar results hold for a rotating black hole.

Suggestive as this was, it appeared in 1973 that there was a puzzling discrepancy. While in certain processes a black hole might behave as though it had a temperature, there was one respect in which it certainly did not, or so it was then thought. I have in mind the very property that gives the black hole its name, namely, its inability to emit radiation. By contrast, an ordinary hot body does of course emit radiation, with an intensity and a spectrum dependent upon its temperature.

This discrepancy was resolved by Hawking in 1974.[16] By using quantum field theory as well as general relativity Hawking was able to show that radiation (and all kinds of elementary particles) would form just outside the horizon of the black hole. Some of the radiation would be able to escape to great distances, where its intensity and spectrum would be precisely the same as for an ordinary hot body at the temperature of the black hole. The link-up with thermodynamics is thereby completed.

This is clearly one of the greatest discoveries ever made in theoretical physics. By uniting quantum field theory, general relativity, and thermodynamics it takes its place alongside the other great unifications in physics which are both conceptually deep and rich in consequences. One thinks of Faraday uniting electricity and magnetism, Clark Maxwell uniting electromagnetism and optics, Einstein

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uniting energy and mass in special relativity and inertia and gravitation in general relativity, and Dirac uniting special relativity and quantum mechanics.

Many consequences of Hawking's work surely remain to be discovered. But one very dramatic one was pointed out by Hawking himself in his first paper on the subject. As a black hole radiates it loses mass/energy (since energy is still conserved in this theory). As a result the black hole gets hotter (since its temperature is inversely proportional to its mass) and so it radiates faster and at higher energies. The final stages are violently explosive, the radiated power during the last tenth of a second being one hundredth of the sun's power output, but coming from an object whose volume is 10^{15} smaller than that of an atom.

Is there any hope of observing such an extraordinary phenomenon? Certainly not for the kind of black hole which may be lurking in Cygnus X–1. Such a black hole would be so cold (about one ten-millionth of a degree absolute) that its radiation rate would be very low and it would take an almost unimaginable time to reach the explosive stage (about 10^{54} longer than the present age of the Universe). However, Hawking has suggested that if black holes of much lower mass could have formed they might have reached the explosive stage by today. Specifically he proposed that conditions in the very early stages of the Universe, close to its big bang origin, might have been conducive to the formation of "mini black holes" of such low mass that they would have reached the explosive stage today 10 billion years later (the mass required is the same as that of a mountain about a kilometer across). Accordingly he has suggested that one should look for such explosions by flying suitable gamma-ray detectors above the Earth's atmosphere. Very recently M. J. Rees[19] has pointed out that it may be easier to detect radio pulses emitted by the electrons and positrons produced in the explosion. Either way, to detect such an explosion would give us priceless information about processes which have no counterpart in terrestrial laboratories, but which represent deep properties of nature.

This discussion also gives added weight to the suggestion made in 1970 by Y. B. Zel'dovich[20] that analogous processes of particle creation by the rapidly changing gravitational field of the early Universe may provide the answer to our questions (2) and (3) relating to the origin of the high symmetry of the Universe and of the 3° K radiation field. Such particle creation processes would be dissipative in character, that is, would tend to smooth out any irregularities that were present initially, and in the process would produce heat which would be rapidly brought into thermal equilibrium with matter.

And so the wheel has come full circle. The 3° K radiation field (a) shows from its observed isotropy that on a large scale the Universe is highly symmetric; (b) tells us directly that the Universe was once dense enough to thermalize it (within 300 years of the big bang); (c) together with observations of the helium abundance enables us to probe conditions within 100 seconds of the big bang; (d) via a singularity theorem tells us that the Universe was once very dense indeed; and (e) may have originated as the heat produced if the high symmetry of (a) has been imposed on the Universe by dissipative processes. A plausible process would involve quantum mechanical particle creation, the counterpart to the creation which would be responsible for the evaporation and ultimate explosion of any mini black holes formed in the early dense regions of the Universe.

When the achievements of contemporary scholarship come to be assessed, it

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should be remembered that all these discoveries were made in just one decade, from 1965 to 1974. I am confident that in the present decade we shall see equally great advances in one of man's profoundest intellectual journeys: to the limits of space and time made manifest in exploding black holes and the origin of the Universe.

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An American Social Science: International Relations

IN THE PAST THIRTY YEARS, international relations has developed as a largely autonomous part of political science. Even though it has shared many of political science's vicissitudes—battles among various orientations, theories, and methods—it also has a story of its own. What follows is an attempt at neither a complete balance sheet nor a capsule history—merely a set of reflections on the specific accomplishments and frustrations of a particular field of scholarship.[1]

Only in America

Political science has a much longer history than international relations. The attempt at studying systematically the patterns of conflict and cooperation among mutually alien actors—a shorthand definition of the subject matter—is recent. To be sure, we can all trace our ancestry back to Thucydides, just as political scientists can trace theirs to Aristotle. But Thucydides was a historian. He was, to be sure, a historian of genius, rightly convinced that he was writing for all times because he was using one particular incident to describe a permanent logic of behavior. Yet he was careful to avoid explicit generalizations, "if . . . then" propositions, and analytic categories or classificatory terms. Modern sociology and political science emancipated themselves from political and social history, political philosophy, and public law in the nineteenth century. International relations did not, even though the kind of social (or asocial) action described by Thucydides never disappeared from a fragmented world, and flourished particularly in the period of the European balance of power. One can wonder why this was so. After all, here was a realm in which political philosophy had much less to offer than it did to those who wondered about the common good in the domestic order. Except for the vast body of Roman Catholic literature preoccupied with just war, and not very relevant to a world of sovereign states, there were only the recipes of Machiavelli; the marginal comments on the international state of nature in Hobbes', Locke's, and Rousseau's writings; some pages of Hume; two short and tantalizing essays of Kant; compressed considerations by Hegel; and oversimplified fragments by Marx. Even so, the little political philosophy that was available should have been sufficiently provocative to make students want to look into the realities. For the philosophers disagreed about the nature of the international milieu and the ways of making it

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more bearable; and they wrote about the difference between a domestic order stable enough to afford a search for the ideal state, and an international contest in which order has to be established first, and which often clashes with any aspiration to justice. Similarly, the contrast between the precepts of law and the realities of politics was sufficiently greater in the international realm than in the domestic realm, to make one want to shift from the normative to the empirical, if only in order to understand better the plight of the normative. Without a study of political relations, how could one understand the fumblings and failures of international law, or the tormented debates on the foundation of obligation among sovereigns unconstrained by common values or superior power? And the chaos of data provided by diplomatic history did not require any less ordering than the masses of facts turned up by the history of states and societies.

Why did a social science of international relations nevertheless fail to appear? The answer to the discrepancy may well be found in that sweeping phenomenon which Tocqueville identified as the distinctive feature of the modern age: democratization. As domestic societies moved from their Old Regimes to their modern conditions—parties and interests competing for the allegiance of large classes of citizens; the social mobilization of previously dispersed subjects; the politics of large agglomerations and unified markets; an increasingly universal suffrage; the rise of parliamentary institutions or plebiscitarian techniques; the fall of fixed barriers, whether geographic or social, within nations—the study of flux began in earnest, if only in order to provide concerned observers and insecure officials with some clues about regularities and predictions of somewhat less mythical, if also less sweeping nature than those grandiosely strewn around by philosophers of history. With democratization, as Comte had predicted, came the age of positivism (his only mistake was to confuse his own brand of metaphysics, or his grand speculations, with positive science). But international politics remained the sport of kings, or the preserve of cabinets—the last refuge of secrecy, the last domain of largely hereditary castes of diplomats.

Raymond Aron has characterized international relations as the specialized activity of diplomats and soldiers. However, soldiers, to paraphrase Clausewitz, have their own grammar but not their own logic. It is not an accident if armies, having been democratized by the ordeals of the French Revolution and Napoleonic era, found their empirical grammarian in Clausewitz, whereas the still restricted club of statesmen and ambassadors playing with the fate of nations found no logician to account for its activities. Indeed, the historians who dealt with these succeeded only in keeping them beyond the pale of the kind of modern science that was beginning to look at societies, by perpetuating the myth of foreign policy's "primacy," isolated from domestic politics. There was, to be sure, one country in which foreign policy was put under domestic checks and balances, knew no career caste, and paid little respect to the rules and rituals of the initiated European happy few: the United States of America. But this country happened to be remarkably uninvolved in the kinds of contests that were the daily fare of other actors. Either it remained aloof, eager merely for continental consolidation and economic growth; or else it expanded, not by conflicts and deals with equals, but by short spurts of solipsistic exuberance at the expense of much weaker neighbors. International relations is the science of the tests and

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trials of several intertwined actors. Where they were intertwined, no science grew. In the United States before the 1930s, there was no reason for it to grow.

It was only the twentieth century that brought democratization to foreign policy. Diplomatic issues moved from the calculations of the few to the passions of the many, both because more states joined in the game that had been the preserve of a small number of (mainly European) actors and (mainly extra-European) stakes, and above all because within many states parties and interests established links or pushed claims across national borders. And yet, a World War that saw the mobilization and slaughter of millions, marked the demise of the old diplomatic order, and ended as a kind of debate between Wilson and Lenin for the allegiance of mankind, brought forth little "scientific analysis" of international relations. Indeed, the rude intrusion of grand ideology into this realm gave a new lease of life to utopian thinking, and delayed the advent of social science. Not "how it is, and why," but "how things should be improved, reformed, overhauled," was the order of the day. Old Liberal normative dreams were being licensed by the League of Nations covenant, while at the same time the young Soviet Union was calling for the abolition of diplomacy itself.

It is against this reassertion of utopia, and particularly against the kind of "as if" thinking that mistook the savage world of the 1930s for a community, the League for a modern Church, and collective security for a common duty, that E. H. Carr wrote the book which can be treated as the first "scientific" treatment of modern world politics: *Twenty Years Crisis*[2]—the work of a historian intent on deflating the pretenses of Liberalism, and driven thereby to laying the foundations both of a discipline and of a normative approach, "realism," that was to have quite a future. Two paradoxes are worth noting. This historian who was founding a social science, did it in reaction against another historian, whose normative approach Carr deemed illusory—Toynbee, not the philosopher of the *Study of History*, but the idealistic commentator of the *Royal Yearbook of International Affairs*. And Carr, in his eagerness to knock out the illusions of the idealists, not only swallowed some of the "tough" arguments which the revisionist powers such as Mussolini's Italy, Hitler's Germany, and the militaristic Japan had been using against the order of Versailles—arguments aimed at showing that idealism served the interests of the status quo powers—but also "objectively," as Pravda would say, served the cause of appeasement. There was a triple lesson here: about the springs of empirical analysis (less a desire to understand for its own sweet sake, than an itch to refute); about the impossibility, even for opponents of a normative orientation, to separate the empirical and the normative in their own work; and about the pitfalls of any normative dogmatism in a realm which is both a field for objective investigation and a battlefield between predatory beasts and their prey.

But it was not in England that Carr's pioneering effort bore fruit. It was in the United States that international relations became a discipline. Both the circumstances and the causes deserve some scrutiny. The circumstances were, obviously, the rise of the United States to world power, a rise accompanied by two contradictory impulses: renewed utopianism, as exemplified by the plans for postwar international organization; and a mix of revulsion against, and guilt about, the peculiar prewar brew of impotent American idealism (as symbolized by the "nonrecognition" doctrine), escapist isolationism (the neutrality laws),

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and participation in appeasement. Two books brought to America the kind of realism Carr had developed in England. Once was Nicholas Spykman's *America's Strategy in World Politics*,[3] which was more a treatise in the geopolitical tradition of Admiral Mahan or Mackinder than a book about the principal characteristics of interstate politics; but it told Americans that foreign policy is about power, not merely or even primarily about ideals, and it taught that the struggle for power was the real name for world politics. The other book was Hans Morgenthau's *Politics Among Nations*.[4] If our discipline has any founding father, it is Morganthau. Unlike Carr, he was not a historian by training; he had been a teacher of international law. Like Carr, he was revolting against utopian thinking, past and present. But where Carr had been an ironic and polemical Englishman sparring with other Englishmen about the nature of diplomacy in the thirties—a discussion which assumed that readers knew enough diplomatic history to make pedantic allusions unnecessary—Morgenthau was a refugee from suicidal Europe, with a missionary impulse to teach the new world power all the lessons it had been able to ignore until then but could no longer afford to reject. He was but one participant in the "sea change," one of the many social scientists whom Hitler had driven to the New World, and who brought to a country whose social science suffered from "hyperfactualism" and conformity the leaven of critical perspectives and philosophical concerns.[5] But he was, among his colleagues, the only one whose interests made him the founder of a discipline.

Eager to educate the heathen, not merely to joust with fellow literati, Morgenthau quite deliberately couched his work in the terms of general propositions and grounded them in history. Steeped in a scholarly tradition that stressed the difference between social sciences and natural sciences, he was determined both to erect an empirical science opposed to the utopias of the international lawyers and the political ideologues, and to affirm the unity of empirical research and of philosophical inquiry into the right kind of social order. He wanted to be normative, but to root his norms in the realities of politics, not in the aspirations of politicians or in the constructs of lawyers. The model of interstate relations which Morgenthau proposed, and the precepts of "realism" which he presented as the only valid recipes for foreign policy success as well as for international moderation, were derived from the views of nineteenth-century and early twentieth-century historians of statecraft (such as Treitschke, and also Weber). Hence the paradox of introducing to the America of the cold war, and of making analytically and dogmatically explicit, notions and a "wisdom" about statecraft that had remained largely implicit in the age to which they best applied, and whose validity for the age of nuclear weapons, ideological confrontations, mass politics, and economic interdependence was at least open to question.

Be that as it may, Morgenthau's work played a doubly useful role—one that it may be hard to appreciate fully if one looks at the scene either from the outside (as does Aron), or thirty years later, as does the new generation of American scholars. On the one hand, his very determination to lay down the law made Morgenthau search for the laws, or regularities, of state behavior, the types of policies, the chief configurations of power; by tying his sweeping analyses to two masts, the concept of power and the notion of the national interest, he was boldly positing the existence of a field of scientific endeavor, separate from history or law. On the other hand, the very breadth of his brushstrokes, the

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ambiguities hidden by his peremptory pronouncements about power, the subjective uncertainties denied by his assertion of an objective national interest, and even more the sleights of hand entailed by his pretense that the best analytic scheme necessarily yields the only sound normative advice—all of this incited readers to react and, by reacting, criticizing, correcting, refuting, to build on Morgenthau's foundations. Those who rejected his blueprint were led to try other designs. He was both a goad and a foil. (Indeed, the more one agreed with his approach, the more one was irritated by his flaws, and eager to differentiate one's own product). A less arrogantly dogmatic scholar, a writer more modest both in his empirical scope and in his normative assertions, would never have had such an impact on scholarship. Less sweeping, he would not have imposed the idea that here was a realm with properties of its own. Less trenchant, he would not have made scholars burn with the itch to bring him down a peg or two. One of the many reasons why Raymond Aron's monumental *Peace and War*[6]—a book far more ambitious in its scope and far more sophisticated in its analyses than *Politics Among Nations*—incited no comparable reaction from scholarly readers may well have been the greater judiciousness and modesty of Aron's normative conclusions. Humane skeptics invite nods and sighs, not sound and fury; and sound and fury are good for creative scholarship. Moreover, Aron's own scholarship was overwhelming enough to be discouraging; Morgenthau's was just shaky enough to inspire improvements.

Still, *Politics Among Nations* would not have played such a seminal role, if the ground in which the seeds were planted had not been so receptive. The development of international relations as a discipline in the United States results from the convergence of three factors: intellectual predispositions, political circumstances, and institutional opportunities. The intellectual predispositions are those which account for the formidable explosion of the social sciences in general in this country, since the end of the Second World War. There is, first, the profound conviction, in a nation which Ralf Dahrendorf has called the Applied Enlightenment,[7] that all problems can be resolved, that the way to resolve them is to apply the scientific method—assumed to be value free, and to combine empirical investigation, hypothesis formation, and testing—and that the resort to science will yield practical applications that will bring progress. What is specifically American is the scope of these beliefs, or the depth of this faith: they encompass the social world as well as the natural world, and they go beyond the concern for problem-solving (after all, there are trial-and-error, piecemeal ways of solving problems): they entail a conviction that there is, in each area, a kind of masterkey—not merely an intellectual, but an operational paradigm. Without this paradigm, there can be muddling through, but no continuous progress; once one has it, the practical recipes will follow. We are in the presence of a fascinating sort of national ideology: it magnifies and expands eighteenth-century postulates. What has ensured their triumph and their growth is the absence of any counterideology, on the Right or the Left, that challenges this faith either radically (as conservative thought did, in Europe) or by subordinating its validity to a change in the social system. Moreover, on the whole, the national experience of economic development, social integration, and external success has kept reinforcing this set of beliefs.

Second, and as a kind of practical consequence, the very prestige and sophis-

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tication of the "exact sciences" were bound to benefit the social ones as well. The voices of gloom or skepticism that lament the differences between the natural world and the social world have never been very potent in America. Precisely because the social world is one of conflict, precisely because national history had entailed civil and foreign wars, the quest for certainty, the desire to find a sure way of avoiding fiascoes and traumas, was even more burning in the realm of the social sciences. The very contrast between an ideology of progress through the deliberate application of reason to human concerns—an ideology which fuses faith in instrumental reason and faith in moral reason—and a social reality in which the irrational often prevails both in the realm of values and in the choice of means, breeds a kind of inflation of social science establishments and pretensions. At the end of the war, a new dogma appeared. One of the social sciences, economics, was deemed to have met the expectations of the national ideology, and to have become a science on the model of the exact ones; it was celebrated for its contribution to the solution of the age-old problems of scarcity and inequality. This triumph goaded the other social sciences. Political science, the mother or stepmother of international relations, was particularly spurred. It was here that the temptation to emulate economics was greatest. Like economics, political science deals with a universal yet specialized realm of human activity. Its emphasis is not on the origins and effects of culture, nor on the structures of community or of voluntary association, but on the creative and coercive role of a certain kind of power, and on its interplay with social conflict. This also drew it closer to that other science of scarcity, competition, and power, economics, than to disciplines like anthropology or sociology, which deal with more diffuse phenomena and which are less obsessed by the solution of pressing problems by means of enlightened central action.

Nations in which this grandiose and activist ideology of science is less overwhelming have also known, after the Second World War, a considerable expansion of the social sciences. But the United States often served as model and as lever.[8] And political science abroad has usually been more reflective than reformist, more descriptive than therapeutic; although, here and in sociology, foreign social scientists reacted against the traditional intelligentsia of moralists, philosophers, and aesthetes by stressing that knowledge (not old-fashioned wisdom) *was* power (or at least influence), they were not driven by the dream of knowledge *for* power. Moreover, when (inevitably) disillusionment set in, it took often far more drastic forms—identity crises within the professions, violent indictments outside—than in the United States. An ideology on probation cannot afford a fall. An ideology serenely hegemonial reacts to failure in the manner of the work horse in Orwell's *Animal Farm*, or of Avis: "I will try harder."

A third predisposition was provided by a transplanted element: the scholars who had immigrated from abroad. They played a huge role in the development of American science in general. This role was particularly important in the social sciences. Here, they provided not merely an additional injection of talent, but talent of a different sort. No social science is more interesting than the questions it asks, and these were scholars whose philosophical training and personal experience moved them to ask far bigger questions than those much of American social science had asked so far, questions about ends, not just about means; about choices, not just about techniques; about social wholes, not just about

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small towns or units of government. So they often served as conceptualizers, and blended their analytic skills with the research talents of the "natives." Moreover, they brought with them a sense of history, an awareness of the diversity of social experiences, that could only stir comparative research and make something more universal of the frequently parochial American social science. In the field of international relations, in addition to Morgenthau, there was a galaxy of foreign-born scholars, all concerned with transcending empiricism: the wise and learned Arnold Wolfers, Klaus Knorr, Karl Deutsch, Ernst Haas, George Liska, and the young Kissinger and Brzezinski, to name only a few. They (and quite especially those among them who had crossed the Atlantic in their childhood or adolescence) wanted to find out the meaning and the causes of the catastrophe that had uprooted them, and perhaps the keys to a better world.

The last two names bring us to politics. And politics mattered. Hans Morgenthau has often written as if truth and power were bound to be enemies (Hannah Arendt has been even more categorical). And yet he shaped his truths so as to guide those in power. The growth of the discipline cannot be separated from the American role in world affairs after 1945. First, by definition (or tautology), political scientists are fascinated with power—either because they want it, at least vicariously, or because they fear it and want to understand the monster, as Judith Shklar has suggested with her usual devastating lucidity.[9] And in the postwar years, what part of power was more interesting than the imperial bit? America the sudden leader of a coalition, the sole economic superpower, the nuclear monopolist, later the nuclear superior, was far more interesting to many students than local politics, or the politics of Congress, or the politics of group pluralism. Almost inevitably, a concern for America's conduct in the world blended with a study of international relations, for the whole world seemed to be the stake of the American-Soviet confrontation. Here was a domain which was both a virgin field for study and the arena of a titanic contest. To study United States foreign policy was to study the international system. To study the international system could not fail to bring one back to the role of the United States. Moreover, the temptation to give advice, to offer courses of action, or to criticize the official ones was made even more irresistible by the spotty character and the *gaffes* of past American behavior in world affairs, by the thinness of the veneer of professionalism in American diplomacy, by the eagerness of officialdom for guidance—America was the one-eyed leading the cripples. Thus, two drives merged, for the benefit of the discipline and to its detriment also, in some ways: the desire to concentrate on what is the most relevant, and the tendency (implicit or explicit) to want to be useful, not only as a scientist, but as an expert citizen whose science can help promote intelligently the embattled values of his country (a motive that was not negligible, among newcomers to America especially). For it was all too easy to assume that the values that underlie scientific research—the respect for truth, freedom of investigation, of discussion, and of publication—were also those for which Washington stood in world affairs.

Second, as I have just said, what the scholars offered, the policy-makers wanted. Indeed, there is a remarkable chronological convergence between their needs and the scholars' performances. Let us oversimplify greatly. What the leaders looked for, once the cold war started, was some intellectual compass

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which would serve multiple functions: exorcise isolationism, and justify a permanent and global involvement in world affairs; rationalize the accumulation of power, the techniques of intervention, and the methods of containment apparently required by the cold war; explain to a public of idealists why international politics does not leave much leeway for pure good will, and indeed besmirches purity; appease the frustrations of the bellicose by showing why unlimited force or extremism on behalf of liberty was no virtue; and reassure a nation eager for ultimate accommodation, about the possibility of both avoiding war and achieving its ideals. "Realism," however critical of specific policies, however (and thus self-contradictorily) diverse in its recommendations, precisely provided what was necessary. Indeed, there was always a sufficient margin of disagreement between its suggestions and actual policies, and also between its many champions, to prevent it from being nothing but a rationalization of cold war policies. And yet the first wave of writings—those of Morgenthau, Wolfers, ur-Kissinger, Kennan, Osgood, Walt Rostow, or McGeorge Bundy—gave both the new intellectual enterprise and the new diplomacy the general foundations they needed. The second wave—roughly, from 1957 to the mid-1960s—turned strategy in the nuclear age into a dominant field within the discipline. This coincided with the preoccupation of officialdom to replace the reassuring but implausible simplicities of massive retaliation with a doctrine that would be more sophisticated; but it also reflected the conviction that force, in a mixture of nuclear deterrence and conventional (or subconventional) limited uses, remained both the most important aspect of power and a major American asset. Here again, in the literature, the attempt at finding principles for any "strategy of conflict" in a nuclear world is inseparable from the tendency to devise a strategy for America, at a time when both sides had weapons of mass destruction, and when there were serious problems of alliance management, guerrilla wars, or "wars of national liberation." A third wave is quite recent: I refer to the growing literature on the politics of international economic relations. It coincides with what could be called the post-Viet Nam aversion for force, and with the surge of economic issues to the top of the diplomatic agenda, caused by a combination of factors: the degradation of the Bretton Woods system, the increasing importance of economic growth and social welfare in the domestic politics of advanced societies, the resurgence of aggressive or protectionist impulses in order to limit the bad effects or to maximize the gains from interdependence, the revolt of the Third World. Once more, the priorities of research and those of policy-making blend.

The political preeminence of the United States is the factor I would stress most in explaining why the discipline has fared so badly, by comparison, in the rest of the world (I leave aside countries like the Soviet Union and China, in which it would be hard to speak of free social science scholarship!). Insofar as it deals primarily with the contemporary world, it seems to require the convergence of a scholarly community capable of looking, so to speak, at global phenomena (i.e., of going beyond the study of the nation's foreign policy, or of the interstate politics of an area) and of a political establishment concerned with world affairs; each one then strengthens the other. When the political elites are obsessed only with what is happening to their country, because it lacks the power to shape what is happening elsewhere, or because this lack of power has

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bred habits of dependence on another state (such as the United States), or because (as in the case of Japan and West Germany) there are severe constraints on the global use of the nation's power, the chances are that the scholars will not have the motivation or receive the impulse necessary to turn individual efforts into a genuine scientific enterprise, and will either turn to other fields with more solid traditions and outlets (such as, say, electoral behavior in France and Britain) or merely reflect, more or less slavishly, and with some delays, American fashions; or else there will be often brilliant individual contributions, but unconnected and unsupported: a Hedley Bull in Australia (and England), a Pierre Hassner in France, to name just these two, do not make a discipline. Even in England and France, which have become nuclear powers, strategic studies have been to a very large extent the preserve of a few intellectual military men, concerned either with reconciling national policy with the predominant doctrines of deterrence, or with challenging these. But the predominant doctrines have remained American, as if even in the more abstract efforts at theorizing about a weapon that has transformed world politics, it mattered if one was the citizen or host of a country with a worldwide writ. Scholars do not like to think about their intellectual dependence on the status of their country, and on the ambitions of its political elite; it disturbs their sense of belonging to a cosmopolitan, free-floating community of science. Even the sociology of knowledge, which has often looked at the debts of scholars to their countries, has been singularly coy about this particular kind of bond. And yet, the link exists. And it is sometimes reinforced by institutional arrangements.

In the case of the United States, there have been three institutional factors that have acted as multipliers of political connection—factors which have not existed, and certainly not simultaneously, elsewhere. One is the most direct and visible tie between the scholarly world and the world of power: the "in-and-outer" system of government, which puts academics and researchers not merely in the corridors but also in the kitchens of power. Actually, it may be wise to distinguish two phases. In the late forties and fifties, those kitchens remained the preserves of the old establishment: a mix of career civil servants, businessmen, and lawyers. They had to cope with the whole world, with a persistent enemy, with the travails of economic reconstruction and the turmoil of nuclear deterrence. They needed both data and ideas, and they turned to the universities. This was the age of the academic as consultant (officially or not), and this was the period in which much research got funded by those departments that had the biggest resources (Defense more than State). The year 1960 was a turning point. Academics became proconsuls and joined the old boys; often they tried to prove that they could cook spicier dishes and stir pots more vigorously than their colleagues. If one had some doubts about "policy scientists," these could only be doubled by the spectacle of scientific policy-makers. Be that as it may, the Washington connection turned an intellectual interchange into a professional one. In countries with a tight separation between the career of bureaucracy or politics and the academic *métier*, such exchanges are limited to occasional formal occasions—seminars or colloquia—and frequent *diners en ville*; but the former tend to be sterile, and the latter hover between witty debates on current affairs, and small talk.

A second institutional factor of great importance is the role of what I have

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elsewhere called the relays between the kitchens of power and the academic salons. The most important of these dumbwaiters is the network of foundations that fed international relations research after the war, and whose role is essential if one wants to understand exactly why the three waves of scholarship coincided so aptly with the consecutive concerns of the statesmen. A combination of intellectual encouragement to "frontiers of knowledge" and civic desire to be of service, the sociological peculiarities of boards of directors composed, to a large extent, of former academics and former officials, the happy accident of vast financial resources that kept growing until the end of the sixties, all this made of the foundations a golden half-way house between Washington and academia. Wasps served in the CIA—pardon, the institution—as well as State; ex-State officials served in the foundations; and even those professors who had some reservations about serving in the government, had no objection to applying to the foundations. It was a seamless pluralism. These precious relays exist virtually nowhere else.

The third institutional opportunity was provided by the universities themselves. They had two immense virtues. They were flexible; because of their own variety, which ensured both competition and specialization, and also because of the almost complete absence of the straitjackets of public regulations, quasi-feudal traditions, financial dependence, and intellectual routine which have so often paralyzed the universities of postwar Europe. The latter got caught in the contradiction between their own past—a combination of vocational training and general education for the elites—and the sudden demands of mass higher education; they could vacillate from confusion to collapse, but the one thing they could rarely do was to innovate. The other virtue of American universities resulted in part from the fact that mass higher education was already a *fait accompli*: they had large departments of political science, which could serve as the matrices of the discipline of International Relations. In France until the late sixties, in Britain until the spread of the new universities, international relations remained the handmaid of law, or the laughingstock of historians; and when political science departments began to mushroom, the other reasons for the development of the discipline in America were still missing. Only in America could a creative sociologist write about the university as the most characteristic institution of the postindustrial age, the laboratory of its discoveries.[10] In other countries, universities are rarely the arenas of research; and when they are, the research funded by public institutions concentrates on issues of public policy which are rarely international—partly for the political reason I have mentioned above, partly because the existence of a career foreign service with its own training programs perpetuates the tendency to look at international relations as if it were still traditional diplomacy. Civil servants obliged to deal with radically new tasks such as urbanization, the management of banks and industries, or housing sometimes think they can learn from the social sciences. Civil servants who deal with so "traditional" a task as national security and diplomacy do not always realize that the same old labels are stuck on bottles whose shapes as well as their content are new. And when diplomats discover that they too have to cope with the new, technical issues of technology, science, and economics, it is to "domestic" specialists of these subjects that they turn—if they turn at all.

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Even in America

If one looks at the field thirty years after the beginning of the "realist" revolution, can one point to any great breakthroughs? The remarks which follow are, of course, thoroughly subjective, and undoubtedly jaundiced. I am more struck by the dead ends than by the breakthroughs; by the particular, often brilliant, occasionally elegant, but generally nonadditive contributions to specific parts of the field, than by its overall development; by the contradictions that have rent its community of scholars, than by its harmony. The specific contributions have been well analyzed in a recent volume of the *Handbook of Political Science*,[11] and I shall not repeat what is said there. If I had to single out three significant "advances," I would list the concept of the international system, an attempt to do for international relations what the concept of a political regime does for "domestic" political science: it is a way of ordering data, a construct for describing both the way in which the parts relate, and the way in which patterns of interaction change. It emerged from the first period I have described above, and continues to be of importance. Next, I would mention the way in which the literature on deterrence has analyzed and codified "rules of the game" which have been accepted as such by American statesmen, and which have served as the intellectual foundation of the search for tacit as well as explicit interstate restraints: MAD ("Mutual Assured Destruction") and arms control are the two controversial but influential offsprings of the doomsday science. Third, there is the current attempt to study the political roots, the originality, and the effects of economic interdependence, particularly in order to establish whether it shatters the "realist" paradigm, which sees international relations as marked by the predominance of conflict among state actors. And yet, if I were asked to assign three books from the discipline to a recluse on a desert island, I would have to confess a double embarrassment: for I would select one that is more than two thousand years old—Thucydides' *Peloponnesian War*, and as for the two contemporary ones, Kenneth Waltz' *Man, the State and War*[12] is a work in the tradition of political philosophy, and Aron's *Peace and War* is a work in the grand tradition of historical sociology, which dismisses many of the scientific pretenses of the postwar American scholars, and emanates from the genius of a French disciple of Montesquieu, Clausewitz, and Weber. All three works avoid jargon; the two contemporary ones carry their erudition lightly: the sweat of toil is missing. How more unscientific can you get?

Let us return to the ideology I alluded to earlier. There was the hope of turning a field of inquiry into a science, and the hope that this science would be useful. Both quests have turned out to be frustrating. The desire to proceed scientifically, which has been manifest in all the social sciences, has run into three particular snags here. First, there was (and there remains) the problem of theory. I have discussed elsewhere at some length the difficulties scholars have encountered when they tried to formulate laws accounting for the behavior of states, and theories that would explain those laws and allow for prediction. A more recent analysis, by Kenneth Waltz, comes to an interesting conclusion: if theory is to mean here what it does in physics, then the only "theory" of international relations is that of the balance of power, and it is unfortunately insufficient to help us understand the field! The other so-called general theories

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are not more than grand conceptualizations, using "confused, vague and fluctuating definitions of variables."[13] This may well be the case; Waltz seems to blame the theorists, rather than asking whether the fiasco does not result from the very nature of the field. Can there *be* a theory of undetermined behavior, which is what "diplomatic-strategic action," to use Aron's terms, amounts to?

Aron has, in my opinion, demonstrated why a theory of undetermined behavior cannot consist of a set of propositions explaining general laws that make prediction possible, and can do little more than define basic concepts, analyze basic configurations, sketch out the permanent features of a constant logic of behavior, in other words make the field intelligible.[14] It is therefore not surprising if many of the theories dissected, or vivisected, by Waltz, are, as he puts it, reductionist, such as the theories of imperialism, which are what he had called in his earlier book "second image" theories (they find the causes of interstate relations in what happens *within* the units); or else, the theories he dismisses were all produced during the first phase—the neophytish (or fetish) stage—of postwar research: the search for the scientific equivalent of the philosopher's stone has been far less ardent in the past twenty years. Waltz' own attempt at laying the groundwork for theory is conceptually so rigorous as to leave out much of the reality he wants to account for. I agree with him that a theory explaining reality must be removed from it and cannot be arrived at by mere induction; but if it is so removed that what it "explains" has little relation to what occurs, what is the use? One finds some of the same problems in all political science; but Waltz is right in stating that international relations suffers from a peculiar "absence of common sense clues": the key variables are far clearer in domestic political systems, whereas here "the subject is created, and recreated, by those who work on it."[15] Still, here as in the rest of political science, it is the fascination with economics that has led scholars to pursue the chimera of the masterkey. They have believed that the study of a purposive activity aimed at a bewildering variety of ends, political action, could be treated like the study of instrumental action, economic behavior. They have tried in vain to make the concept of power play the same role as money in economics. And they have acted as if the mere production of partial theories unrelated to a grand theory was tantamount to failure.

A "science" without a theory may still be a science with a paradigm; and, until recently, the paradigm has been that of permanent conflict among state actors—the realist paradigm. However, in the absence of a theory, a second question has been hard to answer: what is it that should be explained? The field has both suffered and benefited from a triple fragmentation—benefited, insofar as much ingenious research has been brought to each fragment, yet suffered because the pieces of the puzzle do not fit. First, there has been (and still is) the so-called level of analysis problem. Should we be primarily concerned with the international system, that is, the interactions among the units? Or should we concentrate our efforts on the units themselves? There are two conflicting hypotheses behind these strategies. One postulates that the system has, so to speak, some sort of life of its own, even if some of the actors obviously have a greater role than others in shaping and changing the rules of interaction. The other approach postulates that the actors themselves are the strategic level for understanding what goes on among them. One says, in effect: Grasp the pat-

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terns of interaction, and you will understand why the actors behave as they do; the other one says: Look at the actors' moves, and you will comprehend the outcomes. Students of the international system and students of foreign-policy making have never really blended their research. My own conclusion is that of a writer who has worked both sides of the street: I am dissatisfied with each, but I admit that it is hard to be on both at once. The study of the international system provides one with a fine framework, but no more—precisely because the system may well put constraints on and provide opportunities for the actors, but does not "dictate" their behavior; and the study of the actors tells you, inevitably, more about the actors than about the interactions. But what used to be called linkage theory (before linkage became a Kissinger-inspired technique), that is, propositions about the bonds between foreign policy and international politics, has remained in the frozen stage of static taxonomies.

Second, there has also been fragmentation at each level of analysis. One could say, not so flippantly, that each student of international systems has hugged his own version of what that abstract scheme "is." Aron's is not Richard Rosecrance's, which is not Morton Kaplan's. Moreover, each one has tended to look at the postwar international system in a different way (once again, in the absence of a single theory, it is not easy to determine authoritatively the dynamics of a particular system that still unfolds under one's eyes). A dozen years ago, scholars acted as if they were competing for a prize to the best discourse on the subject: are we in a bipolar system? Waltz, Liska, Kissinger, and many others (including me) took part, but since there was no Academy, there was no prize. In recent years, the new contest is about "Persistence or Demise of the Realist Paradigm?": Is the state-centered concept of international politics, with its focus on the diplomatic-strategic chessboard and its obsession with the use of force, still relevant to the age of interdependence? Aron, Joseph Nye and Robert Keohane, Edward Morse, Bull, and many others (including myself) are busy evaluating. As before, I suspect that the verdict will be history's, and that like the long-awaited Orator in Ionesco's *Chairs*, it will speak in incomprehensible gibberish. At the other level of analysis, we have accumulated masses of studies of concrete foreign policies, and moved from the period of Chinese boxes—the decision-making theories of the 1950s—to the age of the "bureaucratic politics" model. The former provided endless items for laundry lists; the other one draws attention to the kitchen where the meal is being cooked, but forgets to tell us that what matters is whether the chefs cook what they want or what they are ordered to prepare, and assumes all too readily that what they do is determined by their particular assignment in the kitchen, rather than by what they have learned outside, or their personal quirks.

Third, there has been functional fragmentation as well. If there is, or can be, no satisfactory general theory, if the "overarching concepts" are excessively loose-fitting clothes, why not try greater rigor on a smaller scale? At the systemic level, we have thus witnessed such clusters of research as work on regional integration (where, for once, the theoretical ingenuity of scholars has far outreached the practical, "real-life" accomplishments of statesmen), modern theories of imperialism, arms race models and measurements of wars, recent studies of transnational relations and international economics. At the foreign policy level (although it tries to straddle both) the main cluster has been that of strate-

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gic literature; and there is now a growing literature on decision-making in the United States. Unfortunately, each cluster has tended to foster its own jargon; and this kind of fragmentation has had other effects, which will be discussed below.

Finally, the quest for science has led to a heated and largely futile battle of methodologies, in answer to a third question: Whatever it is we want to study, how should we do it? Actually, it is a double battle. On the one hand, there is the debate between those "traditionalists" who, precisely because of the resistance the field itself opposes to rigorous theoretical formulations, extol the virtues of an approach that would remain as close to historical scholarship and to the concerns of political philosophy as possible (this is the position taken by Hedley Bull), and all those who, whatever their own brand of theorizing, believe that there can be a political science of international relations—if not in the form of a single theory, at least in that of systematic conceptualizations, classifications, hypotheses, etc.—a science which can be guided in its questions by the interrogations of past philosophers, yet finds reliance on philosophical discourse and diplomatic intuition both insufficient and somewhat alien to the enterprise of empirical analysis. There is little likelihood that this debate will ever come to a conclusion—especially because neither side is totally consistent, and each one tends to oversimplify what it actually does. On the other hand, here as in other branches of political science, there is the battle of the literates versus the numerates; or, if you prefer, the debate about the proper place and contributions of quantitative methods and mathematical models. The fact that the practitioners of the latter tend to hug the word science, and to put beyond the pale of science all those who, while equally concerned with moving "from the unique to the general" and with considering "classes of events and types of entities," believe that these cannot be reduced to numbers or that science does not consist in "accumulating coefficients of correlation" . . . "without asking which theories lead one to expect what kind of a connection among which variables"[16]—this fact has made for rather strained relations among scholars of different methodological persuasions. In domestic political science, behaviorists and old-fashioned scholars have found coexistence easier, because their respective approaches fit separate parts of the field—electoral behavior or the behavior of legislative bodies lends itself to mathematical treatment. In international affairs, such a functional division of labor is much harder to apply. As a result, the prophets of quantitative methodologies dismiss as mere hunches based on "insight" (a word they often use as if it were an insult) the elaborate ruminations of their opponents, and these in turn ridicule the costly calculations that tell one nothing about causes or lump together different types of the same phenomenon (say, wars), and the endless correlations among variables lifted from their context, that all too often conclude that . . . no conclusive evidence can be derived from them: endless nonanswers to trivial questions.

If there is little agreement as to what constitutes a science, and little enthusiasm for the state of the science of international relations, what about the other great expectation, that of usefulness? I am struck by one apparent contradiction. The champions of a science of international affairs have, on the whole, declared their independence from philosophy and their allegiance to objective empiricism. And yet, most of them have wanted to draw consequences for the real

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world from their research: the greater the drive to predict (or the tendency to equate science, not just with intelligibility but with control and prediction), the greater the inclination to play the role of the wise adviser—or of the engineer. It is in the nature of human affairs, and of the social sciences.

But in this specific realm, there are some very peculiar problems. The first could be called: advice to whom? Many scholars, especially those whose level of analysis is systemic, implicitly write as if they were addressing themselves to a world government, or as if they aimed at reaching those who wish to transcend the traditional logic of national self-righteousness and state calculations (the same can be said, even more strongly, of theorists of regional or functional integration; they tend to distribute recipes for going beyong the nation-state). Unfortunately, the chair of World Statecraft is empty, and change comes (if at all) through the operations of state agents. And so, scholars of this kind oscillate from condemnation of state practices that make for conflict, or retard integration, or promote injustice, to advice to state agents on how to transcend the limits of the game which it is however these agents' role and duty to perpetuate, or advice to international secretariats and subnational bureaux on the best strategy for undercutting and turning the resistance of national statecraft. These are all perfect guarantees of unhappy consciousness for the scholars.

Other scholars, especially among those whose level of analysis is national decision-making, see themselves as efficient Machiavellians—they are advising the Prince on how best to manage his power and on how best to promote the national interest. This is particularly the case of the strategists, the group which contains the highest proportion of researchers turned consultants and policy-makers. "Systemic" writers who are fully aware of the differences between an international system and a community of mankind, that is, the "realists," do their best to make advice to the only Prince who still matters—the national statesman, bound to enhance the interests of his state—coincide with their views of the interests of the whole. They advocate "enlightened" concepts of the national interest, or "world order" policies that would somewhat reconcile the needs of the part and of the whole. But this is a difficult exercise. The logical thrust of "realism" is the promotion of the national interest, that is, not unhappy global consciousness but happy national celebration. "Realists" who become aware of the perils of realism in a world of nuclear interconnection and economic interdependence—writers like Morgenthau, or myself—suffer from the addition of two causes of unhappiness: that which afflicts all "systemic" writers in search of a radically new order, and that which comes from knowing only too well that utopianism does not work.

Thus, basically, in their relations which the real world, the scholars are torn between irrelevance and absorption. Many do not like irrelevance, and want even the most esoteric or abstract research to be of use. The oscillation I have described above is what they want to escape from, and yet they do not want to be absorbed by that machine for self-righteousness, the service of the Prince. But their only excuse is the populist dream—the romantic hope that "the people" can be aroused and led to force the elites that control the levers of action, either out of power altogether or to change their ways. Much of peace research, once it got tired of advocating for the solution of world conflicts the discrete techniques used for accommodation in domestic affairs, has been traveling down that route.

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It is one on which scholarship risks finding both irrelevance and absorption, for the policies advocated here do inspire both those intelligentsias that want to displace certain elites in developing countries, and those established elites that are eager to boost national power against foreign dominance. Yet if the former come to power, and if the latter follow the advice of "dependencia" theorists, the result is not likely to be a world of peace and justice, but a world of revolutions, and new conflicts, and new inequities.

As for the scholars who want to avoid esoterica or romanticism and who set their sights on Washington, they, in turn, run into problems. There are two reasons why the Washingtonian temptation is so strong. There is the simple fact that international politics remains the politics of states: whether or not, in the abstract, the actor is the shaper of or is shaped by the system, in reality there is no doubt that the United States remains the most potent player. And there is the fact that a science of contemporary politics needs data, and that in this realm, whereas much is public—in the records of international organizations, speeches, published state documents—a great deal remains either classified or accessible only to insiders: the specific reasons for a decision, the way in which it was reached, the bargains that led to a common stand, the meanderings of a negotiation, the circumstances of a breakdown. Far more than domestic political science, international relations is an insider's game, even for scholars concerned with the systemic level.

But a first problem lies in the fact that gathering information from and about the most potent actor, creates an irresistible urge to nudge the player: the closer the Washingtonian connection, the greater the temptation of letting oneself be absorbed. Second, outsider advice always suffers from oversimplification. When it comes to tactical suggestions, the insiders, who control not only all the facts but also the links connecting separate realms of policy, have the advantage. This increases the scholar's urge to get in closer. Third, once one starts rolling down the slope from research-with-practical-effects, to practical-advocacy-derived-from-research, the tendency to slight the research and to slant the advocacy for reasons either of personal career or of political or bureaucratic opportunity, will become insidious. Which means that the author may still be highly useful as an intelligent and skilled decision-maker—but not as a scholar. Either his science will be of little use, or else, in his attempt to apply a particular pet theory or dogma, he may well become a public danger. This does not mean that the experience of policy-making is fateful to the scholar, that the greatest hope for the science would lie in blowing up the bridge that leads across the moat into the citadel of power. A scholar-turned-statesman can, if his science is wise and his tactics flexible, find ways of applying it soundly; and he can later draw on his experience for improving his scholarly analytical work. But it is a delicate exercise which few have performed well.

Because of America

The problems I have examined have arisen mainly in America, because the profession of international relations specialists happens to be so preponderantly American. Insofar as it flourishes elsewhere, the same difficulties appear: they result from the nature of the field. But because of the American predominance,

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the discipline has also taken some additional traits which are essentially American, and less in evidence in those other countries where the field is now becoming an object of serious study.

The most striking is the quest for certainty.[17] It explains the rage for premature theoretical formulation, the desire to calculate the incalculable (not merely power but status), the crusade to replace discussions of motives with such more objective data as word counts and vote counts, the crowding of strategic research (here, the ends are given, and it becomes a quest for the means). International relations should be the science of uncertainty, of the limits of action, of the ways in which states try to manage but never quite succeed in eliminating their own insecurity. There has, instead, been a drive to eliminate from the discipline all that exists in the field itself—hence a quest for precision that turns out false or misleading. Hence also two important and related gaps. One is the study of statecraft as an art. With very few exceptions (such as *A World Restored*) it has been left to historians. (One could say much of the same about domestic political science). The other is the study of perceptions and misperceptions, the subjective yet essential side of international politics. Robert Jervis' work is beginning to fill that gap, but it is not certain that his example will be widely followed.[18] Almost by essence, the study of diplomatic statecraft and of perceptions refuses to lend itself to mathematical formulations, or to a small number of significant generalizations (one may generalize, but the result is likely to be trivial). Taxonomies and case studies do not quench the thirst to predict and to advocate.

A second feature, intimately tied to the discipline's principal residence rather than to its nature, is the preponderance of studies dealing with the present. Historians continue to examine past diplomatic history in their way. Political scientists concerned with international affairs have concentrated on the politics of the postwar era; and when they have turned to the past, it has all too often been either in highly summary, I would say almost "college outline" fashion, or in the way long ago denounced by Barrington Moore, Jr., which consists in feeding data detached from their context into computers. This is a very serious weakness. It leads not only to the neglect of a wealth of past experiences—those of earlier imperial systems, of systems of interstate relations outside Europe, of foreign policy-making in domestic policies far different from the contemporary ones—but also to a real deficiency in our understanding of the international system of the present. Because we have an inadequate basis for comparison, we are tempted to exaggerate either continuity with a past that we know badly, or the radical originality of the present, depending on whether we are more struck by the features we deem permanent, or with those we do not believe existed before. And yet a more rigorous examination of the past might reveal that what we sense as new really is not, and that some of the "traditional" features are far more complex than we think.

There are many reasons for this flaw. One is the fear of "falling back into history"—the fear that if we study the past in depth, we may indeed find generalizations difficult and categorization either endless or pointless; and we may lose the thread of "science." A related reason is the fact that American political scientists do not receive sufficient training either in history or in foreign languages, indispensable for work on past relations among states. A third reason is

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to be found in the very circumstances of the discipline's birth and development. In a way, the key question has not been, "What should we know?" It has been, "What should we do?"—about the Russians, the Chinese, the bomb, the oil producers. We have tried to know as much as we needed in order to know how to act—and rarely more: a motivation that we find in other parts of political science (the study of political development, for instance), where some disillusionment has set in. But we can say to ourselves that there are no shortcuts to political development, that the United States cannot build nations for others, and that we should go back to the foundations, that is, to an understanding of the others' past. We are unable to say to ourselves that we must stop having a diplomacy, and impose a moratorium on our advising drive until we have found out more about the past of diplomatic-strategic behavior. And the interest which, quite naturally, the government and, less wisely but understandably, the foundations have shown in supporting research that deals with the present (or extrapolates it into the future, or scrutinizes the near future so as to discern what would be sound action in the present) has kept the scholars' attention riveted on the contemporary scene.

The stress on the present and the heavily American orientation have combined to leave in the dark, at least relatively, several important issues—issues whose study is essential to a determination of the dynamics of international politics. One is the relation of domestic politics (and not merely bureaucratic politics) to international affairs—we need to examine in far greater detail the way in which the goals of states have originated, not (or not only) from the geopolitical position of the actors, but from the play of domestic political forces and economic interests; or the way in which statesmen, even when they seemed to act primarily for the world stage, nevertheless also wanted their moves abroad to reach certain objectives within; or the way in which external issues have shaped domestic alignments and affected internal battles. The desire to distinguish the discipline of international relations from the rest of political science is partly responsible for this gap; scholars who study a given political system do not usually pay all that much attention to foreign policy, and the specialists of international politics simply do not know enough about foreign political systems. The only country for which the bond between domestic and external behavior has been examined in some depth is, not so surprisingly, the United States. Here again, an assessment of the originality of the present—with its visible merging of domestic and foreign policy concerns, especially in the realm of international economic affairs—requires a much deeper understanding of the past relations between domestic politics and foreign policy. We may discover that the realist paradigm, which stresses the primacy of foreign policy, has to be seriously amended, not only for the present but for the past.

Another zone of relative darkness is the functioning of the international hierarchy, or, if you prefer, the nature of the relations between the weak and the strong. There has been (especially in the strategic literature) a glaring focus on bipolarity, accompanied by the presumption that moves to undermine it (such as nuclear proliferation) would be calamitous (it may not be a coincidence if the French have, on the whole, taken a very different line). Much of the study of power in international affairs has been remarkably Athenian, if one may refer to the famous Melian dialogue in Thucydides (the strong do what they can, the

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weak what they must). How the strong have often dealt with the weak in ways far more oblique, or less successful than the simple notion of a high correlation between might and achievements would suggest; how and under what conditions the weak have been able to offset their inferiority—these are issues which, until OPEC came along, had not been at the center of research and for which, again, far more historical work ought to be undertaken.

What was supposed to be a celebration of creativity seems to have degenerated into a series of complaints. We have found here an acute form of a general problem that afflicts social science—the tension between the need for so-called basic research, which asks the more general and penetrating questions that derive from the nature of the activity under study, and the desire of those who, in the real world, support, demand, or orient the research, for quick answers to pressing issues. And if the desire often seems more compelling than the need, it is because of the scholars' own tendency to succumb to the Comtian temptation of social engineering. This temptation is enhanced by the opportunities the United States provides to scholar-kings (or advisers to the Prince), or else by the anxiety which scholars, however "objective" they try to be, cannot help but feel about a world threatened with destruction and chaos by the very logic of traditional interstate behavior.

Born and raised in America, the discipline of international relations is, so to speak, too close to the fire. It needs triple distance: it should move away from the contemporary, toward the past; from the perspective of a superpower (and a highly conservative one), toward that of the weak and the revolutionary—away from the impossible quest for stability; from the glide into policy science, back to the steep ascent toward the peaks which the questions raised by traditional political philosophy represent. This would also be a way of putting the fragments into which the discipline explodes, if not together, at least in perspective. But where, in the social sciences, are the scientific priorities the decisive ones? Without the possibilities that exist in this country, the discipline might well have avoided being stunted, only by avoiding being born. The French say that if one does not have what one would like, one must be content with what one has got. Resigned, perhaps. But content? A state of dissatisfaction is a goad to research. Scholars in international relations have two good reasons to be dissatisfied: the state of the world, the state of their discipline. If only those two reasons always converged!

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Process, System, and Symbol: A New Anthropological Synthesis

Just beyond our present horizon, I like to think, lie the Delectable Islands. But I think that anthropologists will reach them only if they reverse the process of fission into subdisciplines, each with its awesome jargon, which has characterized the history of anthropology in the past decade or so, and move toward a renewed fusion which will lead to discussion of an animated sort among, say, the biological, ecological, structural, semiotic, semiological, "etic," "emic," ethno-this and ethno-that, kinds of anthropologists. Our expulsion from various colonial research Edens may be an opportunity rather than a loss. We can now take stock—and also assess our relationship, in this breathing space, to other entrenched sciences and humanities.

Anthropology should not flinch from looking at creative art and literature in complex societies, but always as these reflect upon the "tides of history" which form their processual contexts. Texts not only animate and are animated by contexts but are processually inseverable from them. The arts are germane to the ebbs and flows of human understanding as these awaken or fade at given moments on the scale of global history; the sciences show anthropology the constraints of the human condition. In terms approvable by William Blake, we oscillate between "single" and "fourfold" vision, but neither is inappropriate to us, exhausts us, or cannot be seriously studied.

My personal view is that anthropology is shifting from a stress on concepts such as structure, equilibrium, function, system to process, indeterminacy, reflexivity—from a "being" to a "becoming" vocabulary—but with a tender perpetuative regard for the marvellous findings of those who, teachers of the present generation, committed themselves to the discoveries of "systems" of social relations and cultural "items" and "complexes." The validly new never negates the seriously researched immediate past in any science; it incorporates it in "a wider orbit of recovered law."

As Tom Kuhn and others have shown, the sociopolitical situation in any disciplined field of knowledge, whether classified as a science or not, exhibits the conflicts of processuality (as our Italian colleagues, literally translating, put it). Actual persons represent theoretical stances. Older persons command stronger positions in the micro-comographia academica—persons trained and working valuably in earlier periods than the present. Younger persons are doing the experiments, the fieldwork. Paradigms supported by the "good old boys" are challenged by new facts, new hypotheses grounded in them. Anthropology is presently experiencing this stress rather sharply.

The discipline of anthropology undoubtedly shares this crisis with other academic disciplines. To my possibly naive European eye, stress on the individual as the grant-

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seeking unit, on "chaps rather than maps," has virtues certainly, but it also makes for theoretical fragmentation. And behind it is often a covert, unhealthy collectivism. For certain of the major departments of anthropology in the United States show a kinship to the city states of antiquity. Each department specializes in a certain kind of anthropology (ethnoscience, symbolic anthropology, ecological anthropology, applied anthropology, etc.), and it would be a bold student who successfully obtained support for what his tutors considered an "adversary position" in terms of his grant proposal or thesis research.

This combination of a myth of individualism and a reality of departmental theoretical orientation often tends to create for imaginative students the classical Batesonian double-bind situation. Their best thoughts may be tabooed and their integrity undermined by "city state" shibboleths in the way of concepts and styles to which they must render at least lip service to obtain support from nationally and locally prestigious departmental faculty. Students often seem to suffer from the guilt of "self-betrayal"—which pursues them even into their fieldwork in far places. I am sure this is not an optimal condition for fieldwork. For they have to process their fieldwork into Ph.D. dissertations acceptable by their sponsoring departments.

One remedy would be to seek means to overcome the overspecialization of departments and the atomism of funding. My paper indicates that a new breakthrough in anthropology depends upon a serious sustained effort by the proponents of severely segregated subdisciplines (who bestow on their students the emblems of this segregation as "professional competence") to relate the best findings of their separated years. The major funding agencies, the NSF, NIMH, SSRC, Ford Foundation, etc., should be approached to provide the basis for a series of "summit" meetings among the leaders of the various modes of "anthropologizing." None of the major think-tanks (Palo Alto, Princeton, etc.) has promoted this immense work of collective reflexivity. Not that conferences alone can do this, but they are signals that the reconstitution of anthropology at a higher level under the aegis of processualism is under way. Otherwise the centrifugal drift, indeed, the suicidal sparagmos, *will go on and on.*

The device of encouraging representation on the faculties of certain departments of all the major subfields of current anthropology tends to be a palliative rather than a remedy if it is not cognizantly and authoritatively reinforced by the shared understanding of the discipline that such specialization must be accompanied by authentic integration under a major paradigm whose lineaments have been indicated by the acknowledged creative leaders of the total discipline.

ALTHOUGH IT MAY NOT BE POSSIBLE to point to a definitive breakthrough, exemplified by a single book or article, in the past decade or so in world anthropology, one can make a fair case for a general disciplinary drift toward a theoretical synthesis to which processual studies have largely contributed. However, process theory is no longer linked, as in its earlier heyday, with Gumplowicz's notion that "man's material need is the prime motive of his conduct"[1]; it now recognizes the critical importance of meaning and symboling. Furthermore, its theoretical focus is now "an individual and specific population studied in a multidisciplinary frame of reference and with a stress on specific human behavior rather than generalized norms or averages."[2] Processes of conflict and competition of the social Darwinian type, or of cooperation (accommodation, assimilation) modeled on Kropotkin's zoological "mutual aid," are no longer regarded as being at the dynamic core of social development. Material need is

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not rejected but is rather viewed as part of "the simultaneous interaction among biology, ecology, and culture" which some anthropologists call *biological ecology*.[3]

In this junctural analysis of various systems—not *a* systems analysis but, rather, an *inter*systemic analysis—culture has to be seen as processual, because it emerges in interaction and imposes meaning on the biotic and ecological systems (also dynamic) with which it interacts. I should not say "it," for this is to reify what is, regarded processually, an endless series of negotiations among actors about the assignment of meaning to the acts in which they jointly participate. Meaning is assigned verbally through speech and nonverbally through ritual and ceremonial action and is often stored in symbols which become indexical counters in subsequent situational contexts. But the assignment and reassignment of meaning must be investigated as processes in the domain of resilience possessed by each population recognizing itself to be culturally perduring. For human populations are periodically subjected to shocks and crises, in addition to the strains and tensions of adjustment to quotidian challenges from the biotic and social environments. These involve problems of maintenance of determinate institutional structures as well as of creative adaptation to sudden or persisting environmental changes, making for indeterminacy.

Processual analysis has undoubtedly gained from the phenomenological critique of positivist anthropology. Whereas positivist anthropology held that social phenomena are qualitatively the same as natural phenomena, that the techniques of analysis developed in the natural sciences are applicable with little modification to anthropological investigation, and that anthropology should strive to develop empirically based theoretical propositions which would support predictive statements about social phenomena, the phenomenologists, notably Schutz, insisted that the social world is in many important respects a cultural construct, an organized universe of meaning in the form of what Harold Garfinkel calls a series of "typifications" of the objects within it. Garfinkel follows Schutz and Husserl in their view that "by naming an experienced object, they are relating by its typicality to pre-experienced things of similar typical structures, and we accept its open horizon referring to future experiences of the same type."[4] Garfinkel argues that when a member of a collectivity accounts for his unique actions he is at the same time typifying them in terms of a framework of meaning which he shares with all other members. This framework is what the phenomenological social scientists call *common sense*. Garfinkel argues that even the practical, mundane activities of members of sociocultural groups are "reflexive" at the common sense level, because the social existence of a member's experiences can be established only through their typification, through the effort of relating their uniqueness to the world of meaning generated and transmitted by the group. For these scholars Durkheim's famous attempt to treat social phenomena as "things" is misdirected. What Durkheim fails to do is to analyze the processes, involving shared symbols, gestures, and language, by which social interaction generates an emergent social reality distinct from and external to that of the individuals who produce it. Processual analysis, like phenomenological anthropology, dereifies collective representations into the purposive and cross-purposive actions of

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persons in sequences of negotiations to maintain or retain, modify, or subvert social meanings, even, in some cases, to change the character and structure of common sense. By focusing attention on processes of meaning assignment it may be possible to locate the principles and rules which generate what D. E. Brown has called "presumptively perpetual social units."[5] Brown refers specifically to corporations, which he regards as more readily classifiable than many noncorporate statuses, but like many structural-functionalists, he regards as unproblematical the processes by which corporations and other "surface structures" come into existence, are maintained against disintegrative processes, and are constantly reevaluated by actors. Social meanings cannot be taken for granted. It is not enough to make taxonomies or inventories of jural norms and cultural values, based on formal statements by informants. We have to develop strategies for ascertaining how the actors deal with discrepant norms: what are their standards of appropriateness, how they assess the respective weighings of stated and unstated rules, in short, how they assign meaning to their transactions and interactions.

Processual analysis has recently been considerably advanced by Sally Falk Moore's "Epilogue" to *Symbol and Politics in Communal Ideology*.[6] Moore proposes that "the underlying quality of social life should be considered to be one of theoretically absolute indeterminacy." Such indeterminacy is only partially reduced by culture and organized social life, "the patterned aspects of which are temporary, incomplete, and contain elements of inconsistency, ambiguity, discontinuity, contradiction, paradox, and conflict."[7] She goes, in fact, further than Schutz, Garfinkel, and other phenomenological sociologists, who seem to find some system, vocabulary, and syntax in common sense. Here they share with the structural-functionalists some notion of the priority of determinacy. Moore, however, argues that even where rules and customs exist, "indeterminacy may be produced by the manipulation of the internal contradictions, inconsistencies, and ambiguities within the universe of relatively determinate elements."[8] For Moore, determining and fixing are processes, not permanent states. The seemingly fixed is really the continuously renewed. This model assumes that social reality is "fluid and indeterminate," although regularizing processes continually transform it into organized or systematic forms. These, however, never completely lose their indeterminacy, and can slip back into an ambiguous or dismembered condition unless vigilantly attended. Moore calls the processes in which persons "arrange their immediate situations (and/or express their feelings and conceptions) by exploiting the indeterminacies of the situations or by generating such indeterminacy or by reinterpreting or redefining the rules or relationships, 'processes of situational adjustment.' "[9] A major advance made by Moore in process theory is her proposal that processes of regularization and processes of situational adjustment "may each have the effect of stabilizing *or* changing an existing social situation or order." Both should be taken into account whenever the complex relationships between social life and the continuously renewed web of meanings which is culture are being analyzed. Both types of process contain within themselves the possibility of becoming their schematic opposites, for strategies used in situational adjustment, if often repeated, may become part of processes of regularization. Per contra, if new rules are made for every situation, such rules cannot be said to "regularize" and

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become elements of situational adjustment. The process of creating "legal fictions" may perhaps be regarded as mediating between processes of regularization and situational adjustment.[10]

A caveat should be interpolated here. It has sometimes been forgotten by those caught up in the first enthusiasm for processualism that process is intimately bound up with structure and that an adequate analysis of social life necessitates a rigorous consideration of the relation between them. Historical hindsight often reveals a diachronic profile, a temporal structure in events, but this structure cannot be understood in isolation from the series of synchronic profiles which compose the structure of a social field at every significant point of arrest of the time flow. Processual studies, as Moore has shown us, do not replace a research focus on regularity and consistency. They may, however, give us clues to the nature of forces of systemic maintenance even as they shed light on the countervailing forces of change. When I speak of *structure* here I am well aware of the phenomenological critique of structural-functionalism that it reifies social order and structure. Thus, I am in agreement with David Walsh's comment that "the requirement for a sociological analysis of the problematic character of social order is a suspension of the belief in the facticity of that order so as to concentrate on the routine practices and procedures of interpretation by which members accomplish it in interactional settings."[11] But Walsh's view that what is important is not formal rules but the procedures by which members demonstrate that activities are in accordance with the rule and therefore intelligible seems to me to put too much stress on what Moore would call processes of regularization and not enough on the processes of situational adjustment, which in certain cases may bring about a shift from regularity to indeterminacy. There is much merit in the phenomenological sociologists' argument that in accounting for their actions in a rational way, group members are *making* those actions rational and thus making social life a coherent and comprehensible reality in a way that underlines the constructed nature of all reality—a view particularly developed in the work of Garfinkel, following Schutz (with Husserl shadowing both!). But scientists of "Man," anthropologists in the strict sense, must find "interesting" not only what Garfinkel ironically terms the "uninteresting" "commonplace" events and activities in social life, involving constant negotiations about typifying conduct in endless constructions of common sense, but also what is genuinely interesting, extraordinary, rare—"spare, original, strange." Perhaps anthropologists are in a better position than sociologists in this respect, for their fieldwork is conducted—or has been until recently—mostly among populations having sometimes widely different cultures from their own. The common sense of those whom they study from the outset seems extraordinary though ordinary enough to their subjects. Sociologists, on the other hand, share understandings with their subjects because they share their culture and have to work hard at transforming the taken-for-granted into a fascinating object of study. Anthropologists, sensitized from the outside to the alienness of many of the symbols and meanings shared by those they investigate, often go on to discover what is extraordinary by any reckoning. The profession of anthropology has in its archives so many variant organizations of commonplace everyday activities, every one of which no doubt seemingly exotic or bizarre as apprehended from the standpoint of the others,

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that it is led to probe beneath this surface layer of reflexivity for processes and mechanisms of a generally human type. French structuralism, whose leading anthropological exponent is Claude Lévi-Strauss, attempted this task. Bob Scholte has succinctly summarized Lévi-Strauss's argument: "conscious, empirical, and ethnographic phenomena are assumed to be the concrete and comparable realizations of unconscious, structural, and ethnological systems. These, in turn, are said to be the results of neurological, cybernetic, and physico-chemical universals. Not only does structuralism as a discipline stand or fall on the basis of this premise, but it also provides the paradigmatic closure for the enterprise as a whole: an encompassing movement from the empirical description of ethnographic models on to their final reduction to unconscious, comparable, and universal structures."[12]

Two comments may be made here. First, Lévi-Strauss's date is drawn from completed texts such as myths and culinary recipes. Second, the neurological and physicochemical bases of human behavior are clearly not exhausted by genetically fixed enduring neuronal pathways but have a high potential for innovative behavior. Even if there are inherited genetic structures of cognition, categories or engrams, it is not impossible that at levels of mentality at least as deep there is a capacity for plastic, adaptive, and manipulative behavior in response to changing circumstance. In other words, processual potential may be preconstituted in the physicochemical infrastructure. In any event both structural and processual foundations in biology remain as yet unverified. I wish only to make the point that we are not here dealing with a behavioral surface crawling with processes contrasted with deep unconscious structures. Indeed, it might be possible to reverse the order of depth and regard the structures inferable in collections of myths and kindred phenomena as convenient means of ordering collective experiences arising from the contestation of deep processes of regularization and situational adjustment. Phenomenologists might even argue that structural arrangements (binary logic, split representation, mediation, and the rest) provide boundary conditions for framing that which actors take for granted, "typified conceptions that make up the actor's stock of knowledge, ecological settings, common linguistic usage, and biophysical conditions."[13] From this perspective the structural oppositions and transformations detected by Lévi-Strauss in the "concrete logic" of mythical narratives may not so much provide clues to fundamental cognitive constraints as represent a convenient and simplistic coding of items of common sense knowledge. We must look elsewhere for intimations of human depth. It is here that we must turn once more to the investigation of processes, but now to processes heavily invested with cultural symbols, particularly those of ritual, drama, and other powerful performative genres.

Van Gennep was the first scholar who perceived that the processual form of ritual epitomized the general experience in traditional society that social life was a sequence of movements in space-time, involving a series of changes of pragmatic activity and a succession of transitions in state and status for individuals and culturally recognized groups and categories. Certainly he was ahead of his time; other investigative procedures had to be developed before his discovery could become the foundation of salient hypotheses. He might be compared with Hero of Alexandria, who described the first known steam

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engine in 120 B.C. Unlike James Watts's model nineteen hundred years later it performed no useful work, merely causing a globe to whirl, but not a world of invention to turn! Van Gennep, a folklorist, had what he considered an almost mystical inspiration as he attempted to elicit the processual structure of two types of rite: those which mark, and, in indigenous thought, bring about the passage of an individual or social category from one cultural state or social status to another in the course of his, her, or their life cycle; and those which mark culturally recognized points in the passage of time (first fruits, harvest, midsummer, new year, new moon, solstice, or equinox). He found that *rites de passage*, viewed cross-culturally, had three principal stages: rites of separation, margin (or *limen* = threshold), and reaggregation. The duration and complexity of these stages varied according to type of rite, though initiatory rites tended to have a protracted liminal stage. Max Gluckman has taken Van Gennep to task for stressing the mechanisms of ritual rather than the role which "whole ceremonies and specific rites play in the ordering and reordering of social relations."[14] However, descriptive social anthropology had not in Van Gennep's time provided the holistic characterization of social systems which would have made this possible, whereas the coolness displayed by Durkheim and his school to Van Gennep's work must have discouraged Van Gennep from attempting to relate his processual discovery to the early structural-functionalist formulations of the *Année sociologique* group. American scholars were among the first to note the theoretical significance of Van Gennep's discovery. As early as 1942 E. D. Chappell and C. S. Coon had attempted to discuss his analysis of rites of passage in a framework of equilibrium-maintenance theory, and had added a fourth category, "rites of intensification," which had as their main goal the strengthening of group unity.[15] J. W. Whiting and I. L. Child,[16] Frank W. Young,[17] and Solon T. Kimball[18] are among those scholars who have in recent years seen the relevance of Van Gennep's formulation for their work in varied fields. Kimball has noted how Van Gennep went beyond his analysis of the triadic processual structure of rites of passage "to an interpretation of their significance for the explanation of the continuing nature of life." Van Gennep, continues Kimball, believed that rites of passage with their symbolic representation of death and rebirth illustrate "the principles of regenerative renewal required by any society."[19] The present author, stimulated during his fieldwork by Henry Junod's use of Van Gennep's interpretative apparatus for understanding Thonga ritual,[20] came to see that the liminal stage was of crucial importance with regard to this process of regenerative renewal. Indeed, Van Gennep sometimes called the three stages "preliminal, liminal, and postliminal," indicating that importance. But he never followed up the implications of his discovery of the liminal beyond mentioning that when individuals or groups are in a liminal state of suspension, separated from their previous condition, and not yet incorporated into their new one, they present a threat to themselves and to the entire group, requiring their segregation from quotidian life in a milieu hedged around by ritual interdictions. In 1963, while awaiting a visa to live in America, suspended between cultural worlds, I wrote a paper, later to be published in the *Proceedings of the American Ethnological Society for 1964*, whose title expresses what for me is the distinctive feature of liminality: "Betwixt and Between: The Liminal Period in *Rites de Passage*."[21] "Liminars," who may be

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initiands or novices in passage from one sociocultural state and status to another, or even whole populations undergoing transition from one quadrant of the solar year to another in a great public ceremony, are "neither here nor there"; they are betwixt and between the positions assigned and arrayed by law, custom, convention, and ceremonial. Van Gennep pointed to the many symbols of birth, death, and rebirth found in the liminal stage in many societies and religions. But for me the essence of liminality is to be found in its release from normal constraints, making possible the deconstruction of the "uninteresting" constructions of common sense, the "meaningfulness of ordinary life," discussed by phenomenological sociologists, into cultural units which may then be reconstructed in novel ways, some of them bizarre to the point of monstrosity (from the actors' own "emic" perspective). Liminality is the domain of the "interesting," or of "uncommon sense." This is not to say that it is totally unconstrained, for insofar as it represents a definite stage in the passage of an initiand from status A to status B in a ritual belonging to a traditional system or sequence of rituals, liminality must bear some traces of its antecedent and subsequent stages. To use Robert Merton's terms, some symbols must accord with the "manifest" purposes of the ritual (to transform a boy into a man, a girl into a woman, a dead person into an ancestral spirit, etc.). But others have the "latent" capacity to elicit creative and innovative responses from the liminars and their instructors. The study of masks and costumes in African and Melanesian initiation rituals, whether of puberty or into secret societies, demonstrates the imaginative potential unlocked by liminality, for maskers (representing deities, arch-ancestors, territorial guardian spirits, or other supernaturals) typically appear in liminal sites sequestered from mundane life. Among the Ndembu of Zambia, for example, the *makishi*, masked figures, said to be ancient ancestors, of awesome shape and power, are believed by boy novices secluded in the bush camps during the *Mukanda* rites to spring from the blood-soaked site in the deep bush where they had recently been circumcised. The woodcarvers who create the masks, though they portray a limited range of types (the Foolish Young Woman, the Crazy One, the Wise Old Chief, the Fertility Binder, etc.), display a wide range of personal aesthetic initiative in generating variant forms.

In other words, there is an aspect of play in liminality. Huizinga's *Homo ludens*[22] has sensitized anthropological thought to the play element in the construction and negotiation of meaning in culture, by his scrutiny of all kinds of playing from children's games to the dialectic of philosophy and the judicial process. After him, play is a serious business! Not all play, of course, is reserved, in any society, for liminal occasions in the strict Van Gennepian sense of ritual stages. In tribal societies, children and adults play games in nonritual, leisure contexts. But the serious games which involve the play of ideas and the manufacture of religiously important symbolic forms and designs (icons, figurines, masks, sand-paintings, murals in sacred caves, statues, effigies, pottery emblems, and the like) are often, in traditional societies, reserved for authentically liminal times and places. In "Betwixt and Between" I invoked William James's "law of dissociation" to clarify the problem of liminal monsters—so often presented through different media to initiands. James argued that when *a* and *b* occurred together as parts of the same total object, without being

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discriminated, the occurrence of one of these, *a*, in a new combination, *ax*, favors the discrimination of *a*, *b*, and *x* from one another. As William James himself put it in his *Principles of Psychology*: "What is associated now with one thing and now with another, tends to become dissociated from either, and to grow into an object of abstract contemplation by the mind. One might call this the law of dissociation by varying concomitants."[23] Liminal monsters and dragons are compounded from various *discriminata*, each of them originally an element in the common sense construction of social reality. In a sense, they have the pedagogical function of stimulating the liminars' powers of analysis and revealing to them the building blocks from which their hitherto taken-for-granted world has been constructed. But in another way they reveal the freedom, the indeterminacy underlying all culturally constructed worlds, the free play of mankind's cognitive and imaginative capacities. Synthesis, as well as analysis, is encouraged by monster construction! In many cultures, liminality is often the scene for immolative action which demonstrates, usually subverbally, this innovative freedom. Symbolic structures, elaborately contrived, are exhibited to liminars at most sacred episodes in the marginal rites, and are then, despite the time and labor taken to construct them, destroyed. Shakespeare's "cloud-capped palaces," as his master of liminality, Prospero, declared, "leave not a rack behind." The fabrications of liminality, being free from the pragmatics of the common sense world, are "baseless fabrics of this vision"—like *The Tempest* itself. Yet the products of ritual and dramatic ritual are surely not ineffectual—at least they survive in ways not altogether to be expected. How, then, should they be assessed in the terms of sociocultural science?

Let me advert to the ecological anthropological concept of resilience mentioned above.[24] This holds that ecological systems (including those ordered by culture) survive "in so far as they have evolved tactics to keep the domain of stability, or resilience, broad enough to absorb the consequences of change." Here we are not only once more in the presence of the constant negotiation of meanings of the phenomenological anthropologists, and the mutually modifying processes of regularization and situational adjustment elicited by Sally Moore, but we begin to see the evolutionary, and, indeed, mere survival, value of the cultural carving out by negotiation, over the ages of sapient development, of ritualized spaces and times, given over as most sacred, privileged, and inviolable moments, to the manufacture of models for behavior and conduct, even if the cost is oftentimes the production of weird and extravagant forms. Evolutionary theory, since Darwin, has always stressed the importance of variability, as it permits a given species to adapt to changing conditions; and selection, at the zoological level, is often for variability rather than for homogeneity, which demands a single limited environment. Anthropologists such as Malinowski and Lévi-Strauss have always found the distinction between nature and culture a useful one, and I share their conviction. I see liminality, in tribal societies, even when they have inhabited a single ecological environment for a long time by any measure, as the provision of a cultural means of generating variability, as well as of ensuring the continuity of proved values and norms. This is done sometimes by mirror inversion of mundane life, so that liminars become the simple antitheses of their antecedent secular "selves" (the bundle of roles occupied preritually). Thus, in many puberty rites boys are invested symbolically with

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feminine attributes, and girls with masculine traits, on the way from juniority to seniority in social classification, or political authority.[25] With increasing frequency, however, inversion gives way, as societies increase in scale and complexity, to the liminal generation of many alternative models.

It should be mentioned, however, that, like everything else in liminality, the primacy of play does not go uncontested, especially in societies that have been long in one place and have had time to consolidate their common sense structures into plausible semblances of "natural systems." Here, in the dangerous realm of challenge to all established jural-political, kinship, and other structures, it is necessary, above all, to maintain, even through the liminal stage, a strong thread of "common sense-ically" constructed order. This is often represented by *sacra*, carefully concealed and seldom revealed symbols and configurations of symbols, which are exposed only on rare liminal occasions. They represent the axiomatic rules and definitions of the culture—usually one that has been well consolidated by continuous occupancy of a single territory over a goodly period of time. In such relatively homogeneous cultural groups where understandings are widely and deeply shared, the liminal periods of ritual have episodes in which the axioms and principles which govern mundane life have solemn representation in myths and symbols which do not contravene or criticize the mundane order (as ludic construction often do) but present it as based in the primordial cosmogonic process. Here ritual is less play than work, and the culture that has such liminality tends to be nonreflexive as well as nonadaptive. Where religious systems and their rituals are backed by superior political force and power they tend to lose their ludic innovativeness and variability, for the success of a single form tends to reduce the need to "carry" a store of alternative forms. That this may be shortsighted is seldom recognized, for climatic, geological, or historical processes may bring disasters and traumas for which the single-model ritual system, represented by a set of paradigmatic myths and their ritual translation into action, may provide inadequate models for mental and physical response. What, paradoxically, may be more functional is a culture which carries with it over time a store of seemingly nonfunctional, even ridiculous liminal schemata for behavior, in evolutionary terms, a repertoire of variant deep cultural models, one of which may prove to be adaptive in drastically changed ecological and biotic conditions. It may even be said that when structure, in the French cognitive sense, penetrates the partially ludic space-time of liminality, imposing on it a "grammar and lexicon" of rules found to be successful in mundane, extraritual contexts, the society thus beset has so much the less adaptive resilience.

Another feature of liminality is that it may be said to contain at least one, and probably more than one, "metalanguage." The term is Gregory Bateson's, a thinker who, if it can so be claimed for anyone, has given the social sciences a new way of talking about the phenomena and processes we study.[26] Bateson talks about "a play frame which is involved in the evaluation of the messages which it contains."[27] He argues that in human systems of communication, and also in many nonhuman, zoological systems, certain signals are emitted in relations among actors, which frame the subsequent proceedings for a variable period of time, in which communication is not direct but which is about the forms of communication used in the day-to-day processes of ensuring survival—

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in mankind's case, the productive relations and the forms of social control guaranteeing their orderliness and relative freedom from conflict. Metacommunication is self-conscious, but plurally and cumulatively self-conscious. It is the way a population or group evaluates its own routine behaviors. Because it is collective and cumulative it perhaps lacks the trenchancy of individual commentary, but compensates by its positing of generic thought against generic experience. Because it represents the reflexivity of many it has perforce to clothe itself in multivocal ("susceptible of many meanings") symbols, and against the univocal signs in which the logical thought of gifted individual philosophers is expressed. We have the plural self-consciousness of men experiencing and thinking together as against the singular self-consciousness of a master craftsman of cognitive reflexivity. Plurality brings feeling and willing (*orexis*) into the act. One might even argue that the founders of major religions (whose adherents still can be counted in hundreds of millions, "objectively"—hence "scientifically"—speaking) occupied a medial position between tribal (and plural) reflexivity and industrial (and singular) reflexivity (as represented by the Western European thinkers), in that they spoke for collectivities and their common sense values, and, at the same time, provided their critique; whereas the Western philosophical tradition, losing much of the plural, social component, spoke for the individual, cognitively liberated though orectically alienated, as against the "damned compact majorities" of Ibsen's Dr. Stockman in *An Enemy of the People*.

If liminality is tribal, traditional ritual is a mode of plural, reflexive, often ludic metacommunication (though containing the countervailing processes and symbols of system maintenance), we have to ask the question—whether it can be satisfactorily answered or not is another set of questions for investigation—what are the functional equivalents of liminality in complex societies, high on the dimensions of scale and complexity, with ever increasing division of labor and specialization of crafts and professions, and where the concept of the individual as against the mass is positively evaluated?

Before we can answer this question we might consider the distinction between indicative and subjunctive moods of verbs—those classes of words expressing action, existence, or occurrence (for we are concerned with the processual aspects of nature and culture). The indicative mood commonly designates the expression of an act, state, or occurrence as "actual"; it asks questions of "fact"—in terms of the definitions of tested facts acceptable in the common sense world of a given human population. Where the subjunctive mood is found, it tends to express desire, hypothesis, supposition, possibility: it may or might be so. In its expressive range it embraces both cognitive possibilities ("hypotheses" = unproved theories or propositions tentatively accepted to explain certain facts or relations) and emotional ones (though here the optative mood might be a better appellation, because it expresses wish or desire). Enacted fantasies, such as ritual and carnival disguises, probably belong here. At any rate one might classify ordinary, quotidian life as indicative, even much of ceremonial or ritual. But one would have to reckon liminal processes subjunctive or optative, for they represent *alternatives* to the positive systems of economic, legal, and political action operating in everyday life. But if the indicative is "bread," mankind "does not live by bread alone." It seems that the dialectic between *is* and *may be*, culturally elaborated into the distinction

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between pre- and post-liminal, on the one hand, and liminal, on the other, forms a continuous human social process, involving biological, ecological, and sociocultural factors, and made reflexive by the search for meaning raised above common sense to a higher power.

The great social thinkers have indicated a drift, trend, or direction in cultural history, which, whether it may be called progressive or regressive, at any rate indicates a series of linear developments. Henry Maine hypothesized a modal shift "from status to contract," Durkheim a move from "mechanical" to "organic" solidarity; Marx and Engels postulated a stage of "primitive communism" subverted by the development of productive forces which generated class oppositions around the issue of property. Private property, in its turn, generated the notion of individuality and elevated it to philosophical respectability, even as it assured the impoverishment and alienation of the masses of mankind—who had no property other than their "labor-power" to put to use in earning a living. They predicted, in Hegelian fashion, an abolition of private property and class ownership of the means of production by a regenerated communism among the propertyless masses, organized ironically—and dialectically—by the very means of production from which the ruling classes derived their profits, which, directed by an elite cognizant of the laws of historical development, that is, the Communist Party, would overthrow the instruments of class hegemony, army and police forces, and restore at a higher material level the primordial communism which was mankind's best preclass state.

Major social thinkers have posited a developmental sequence in types of sociocultural systems in which emphasis shifts from the collectivity as the effective moral unit to the individual. This is paralleled by a growing stress on achieved as against ascriptive status, and by an ever more precise division of labor in the domains of economics and social control. The obligatory component in social relationship yields to the optional or voluntaristic, status gives way to contract. Corporate groups constructed on the model of kin ties are replaced as effective social centers of action by associations of those having like interests. Rational and bureaucratic organization lords it over groups bonded by ties to locality. The city prevails over the rural hinterland. Industrialization has decisively split work from leisure by its "clocking in and out" devices and has reserved play for the leisure sphere wherein work is complemented or rewarded.[28] Ritual which, particularly in its liminal stage, contained both work and play (many tribal societies speak of ritual activity as work, for ceremonies are part of the ongoing process of the whole group) now becomes a leisure activity. Moreover, its liturgical structures accentuate the solemn and attenuate the festal aspects, as codes for moral behavior becomes increasingly internalized as "conscience." When it loses its capacity to play with ideas, symbols, and meanings, when it loses its cultural evolutionary resilience, ritual ceases to be an effective metalanguage or an agency of collective reflexivity.

Some anthropologists, and scholars in adjacent fields, influenced by developments in adjacent disciplines, such as history and literary criticism, are beginning to turn their attention to both the folk and high culture of complex societies and civilizations (C. Geertz, M. Douglas, R. Firth, J. Peacock, B. Myerhoff, and R. Grimes spring readily to mind). In these they find that the obligatory rituals and ritualized bonds characteristic of complex, rurally based

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civilizations have been supplanted by city-based associational and professional linkages. The dismemberment of ritual has, however, proved the opportunity of theater in the high culture and carnival at the folk level. A multiplicity of desacralized performative genres have assumed, prismatically, the task of plural cultural reflexivity. The *sparagmos* (dismemberment) of major liturgical systems, or, in some cases, their relegation to the periphery of the social process, has resulted in the genesis and elaboration of esthetic media, each of which takes as its point of departure a component subgenre of traditional ritual. Thus the dramatic scenario—frequently the enactment of a sacred narrative—now becomes a performative mode sui generis breeding a multiplicity of plots most of which are far from sacred! Song, dance, graphic and pictural representation, these and more, broken loose from their ritual integument, become the seeds of concert music, ballet, literature, and painting. If ritual might be compared to a mirror for mankind, its conversion into a multiplicity of performative arts gives us a hall of magic mirrors, each reflecting the reflections of the others, and each representing not a simple inversion of mundane reality, but its systematic magnification and distortion, the ensemble composing a reflexive metacommentary on society and history as they concern the natural and constructed needs of humankind under given conditions of time and place.

The fragmentation of a collective liturgical work, such as ritual, paves the way for the labeling of specific esthetic works as the production of individuals. But, in fact, all performative genres demand an audience even as they abandon a congregation. Most of them, too, incarnate their plots or scores in the synchronized actions of players. It is only formally that these esthetic progeny of ritual may be described as individual creations. Even such forms as the novel involve a publishing process and a reading process, both of which have collective and initiatory features. A great opportunity is opening up for scholars in both social sciences and humanities who are interested in the reflexive or dialectical relationship between common sense processes in the "getting and spending" (biocultural-ecological) dimensions of sociocultural life and the popular and high performative genres which continually scrutinize, criticize, subvert, uphold, and attempt to modify the behavior of the personnel, their values, activities and relationships, centrally concerned with the maintenance and management of those processes; or which make statements, in forms at least as bizarre as those of tribal liminality, about the quality of life in the societies they monitor under the guise of "entertainment"—a term which literally means "holding between," that is, "liminalizing." Instead of studying socioeconomic processes in isolation from these "magic mirrors," or dramatic types as texts in vacuo, it is possible to envision a creative collaboration among literary critics, anthropologists, sociologists, historians, art historians, philosophers, historians of religion, and other kinds of scholars, on the shared field of the relationship, say, between Noh, Kowaka, Kabuki, Kyogen, Bunraku, and other Japanese theatrical genres, and the social and cultural history of Japan at the time of genesis and in the successive periods of development and decline of these genres—always with the stress on the reciprocal relationship between social process and dramatic medium under varying conditions of time and place. A formidable undertaking? But a great one. The same might be said of dynamic studies of Elizabethan and Stewart drama, Greek high and low comedy, the commedia dell' arte, the

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theater of the absurd, and the theater of cruelty, not merely in sociocultural context but in live reflexive relationship to the fluctuant problems of their times—in terms of competent, dynamic sociological and anthropological analysis, both synchronic and diachronic.

In a recent appraisal of modern social anthropology Sir Raymond Firth has commented on its inward-turning disposition in the sense that modern anthropology is concerned not solely with the behavior of the populations investigated but mainly with their models for perceiving and interpreting their materials and generating their behavior, with their modes of thought, not modes of action.[29] The position that I am presenting, on the contrary, stresses attention to the relationship between modes of thought and of action. Furthermore, if models are to be considered, I would draw on recent work on the role of metaphor in assigning meaning to social behavior, conduct, and action, and argue that, at least implicitly, many sociocultural systems, insofar as they may be considered to be systems, are oriented, through the cumulative effect of their performative genres, to what I have called *root paradigms*. These are not merely cognitive clusters of rules from which many kinds of social actions can be generated, but represent consciously recognized (though only on occasions of raised consciousness) cultural models of an allusive, metaphorical kind, cognitively delimited, emotionally loaded, and ethically impelled, so as to give form to action in publicly critical circumstances. Such root paradigms are often based on generally accepted narratives of climaxes in the careers of religious or political leaders, having thus an existential rather than merely morally edifying character, and often emphasize the primacy of social over individual goals when choices appear between these in extreme situations, to the point of endorsing personal sacrifice for others. Key decisions in the lives of religious founders, such as Gautama, Moses, Jesus, and Mahomet, and of political leaders such as Lenin and Gandhi, are portrayed as exemplary through a wide range of performative genres, and form an almost engrammatic component of socialization. Biocultural anthropologists might regard this stress on sacrifice of self for society as possessing survival value at the transcultural species level. When we shift our perspective to that of the performative genres, we may see these paradigmatic acts in terms of such "loaded" values as supreme love, compassion, and heroism. It may be that our future task as scientists of the human condition is to establish a set of concepts occupying a liminal ground between objective estimation of values promoting species survival and subjective response to stirring exemplifications of self-sacrifice for group survival.

If one is to be as bold as the editor of *Daedalus* would have us in assessing whether or not breakthroughs have happened in anthropology, one would have to record that the potentiality for a major breakthrough exists today. It may be that such a breakthrough is possible only in the United States, for British, French, and other European and third world anthropology, partly through the limited number of their practitioners, tend to be more homogeneous and committed to the pursuit of agreed-upon goals—structuralism and neo-Marxism in France, sophisticated structural-functionalism and conflict theory in Britain, and the political evaluation of all established metropolitan theories in the Third World. It is only in the United States that a "thousand flowers" have truly blossomed in a rigorous theoretical way, because of the huge size and cognitive

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individualism of the subcontinent, where each major department may be likened to an autonomous Hellenic city state—where each, through the diaspora of its graduates, has a nimbus of satellites, both individuals and groups. The outcome has been that a number of perspectives on the human condition, each technically and theoretically of excellent quality, have sprung up in virtual independence of one another. One thinks at once of cultural anthropology, social anthropology, symbolic anthropology, ecological anthropology, biocultural anthropology, phenomenological anthropology, structuralist anthropology, biocultural ecology, legal and political anthropologies, plus the many hybridizations between anthropology and other scholarly approaches: anthropological linguistics and ethnography of speaking; the uses of systems theory in archeological research; Marxist approaches in anthropology; applications of the sociology of knowledge to anthropological data; and others.

My suggestion is that, instead of working in blinkers, anthropologists and scholars in adjacent disciplines interested in cross-cultural problems, should make an earnest (and "ludic") attempt at mutual empathy—earnest in the sense that the disciplines mentioned above, and significant others, might be treated at least as a unified field whose unity might have something to do with the systems theory view that there are systems and systemic relations so fundamental that they occur in many different living and even inorganic phenomena. This would not be to reduce the distinctive features of the disciplines entering into the field to some bland interactional average, but would respect the natural independence of each within their dynamic interdependence. It would also represent a struggle against disciplinary chauvinism. For example, vulgar Marxists and others who have placed their faith in the primacy of economic forces and relations, could not imperialistically claim that ways of thinking about and appreciating man's relationship to the cosmos and his fellows were, by definition, a transmitted load of "false consciousness"—false at least until the defeat of all adversaries of the proletariat. Rather, they should hold the evaluation of the nature and magnitude of productive forces and the conflicting classes resulting from them as at least as problematic as "ideology." Why so much, so fast, and so wastefully employed? The perspectival view from the infrastructure may be at least as false as any superstructural cosmology. What is required is a firm, scholarly, yet imaginative grasp of the total phenomena produced by Man alive and Woman alive. It is strange that both the Hegelian and Marxist logics should conceptualize dialectic so uncompromisingly from the positive and structuralist position of thesis. It is not so much a question of the content of a process of self-transformation being made up of opposing factors or forces as of any cognitive or jurally normative structuration of human processes and relationships encountering, as the indicative confronts the subjunctive mood in verbs, a virtually unlimited range of alternative ways of doing things or relating people. Limits may be set, of course, by biotic or ecological and often by historical conditions. But for the dialectical negation we should perhaps substitute liminality, a plurality of alternatives rather than the reversal or inversion of the antecedent condition. Moreover, the motor of historical dialectic is not so much a matter of quantitive increments cumulating to a qualitative change as deliberate formulations of human thought and imagination—often made in liminal situations, such as exile, prison, or even in an "ivory tower"—first presenting, and then perhaps

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backed up by organized action, a new vision. Liminality is a major source of change rather than the embodiment of a logical antithesis. Science is not mocked—but then neither is art. If what has been durably regarded as the "interesting" by the informed opinion of thousands of years of human attention cannot be incorporated into the serious study of mankind, then that study is surely in the hands of the "philistines"—the "bourgeois and the bolshevik" of D. H. Lawrence—who were so intent on securing by force general assent to their opposed views on the nature of material property (one said "private" should be the basic label, the other "public") that the richness and subtlety of human "immaterial" culture (especially, one might add, its liminal constructions) escaped this Tweedledum-and-Tweedledee pair of dedicated materialists.

What is needed in anthropology is work under the aegis of a *wider orbit of recovered law* in which specialists in its hitherto separate subdisciplines, biological, ecological, social, and cultural anthropology, utilize systems theory to integrate their finds and research procedures in a single field, stress the primacy of processual approaches, incorporate what phenomenologists have to say about the negotiation of meaning, and remain aware of the powerful role of sociocultural liminality in providing conditions for reflection, criticism, rapid socialization, the postulation of variant models of and for conduct and social organization, and the reformulation of cosmologies religious and scientific.

Signs of renewed interest in processual and systems theory abound in the recent literature. If one glances through the articles in *Annual Review of Anthropology* for 1975 one finds Jane F. Collier writing: "Legal processes are social processes. Law . . . is an aspect of ongoing social life," etc., and her article is peppered with items of "process" vocabulary and with references to the legal handling of conflict as framed by extended-case analysis.[30] E. A. Hoebel in the United States[31] and Max Gluckman in Britain[32] may be said to have been among the pioneers of processual analysis through their studies of law as social process, as Collier recognizes. Fred T. Plog in "Systems Theory in Archeological Research" shows how processual thought is influencing the new archeology where it is intrinsically linked to general systems theory: "The interest in general systems theory in archeology has been expressed primarily by 'processual archeologists' and has been a component of the 'systemic approach' that these archeologists have advocated."[33]

A. P. Vayda and B. J. McCay, in their essay "New Directions in Ecology and Ecological Anthropology" in effect support Sally Moore's view that processes of regularization, processes of adjustment, and the factor of indeterminacy must be taken into account in studying sociocultural populations, when they attempt to rescue the notion of *homeostasis* from its previous association with concepts of static equilibria and unchanging systems.[34] They cite Slobodkin as emphasizing that "some properties of homeostatic systems must at times change so as to maintain other properties that are important for staying in the existential game—. . . e.g. resilience and what might be described as flexible enough to change in response to whatever hazards and perturbations come along."[35] Systems theory, in fact, can be modified to handle the irruption of sudden unprecedented changes, making it processually viable and disencumbering it from those structural-functional assumptions which metaphorized sociocultural systems either as organisms or machines.

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The present author has for some time tried to analyze ritual processually in a number of settings, ranging from African traditional societies to medieval and modern pilgrimages in several universalistic religions. Ritual studies led him into the analysis of ritual symbols and, later, of social symbols in general. This type of investigation, which is sometimes called *processual symbolic analysis*, is concerned with the interpretation of the meaning of symbols considered as dynamic systems of signifiers, signifieds, and changing modes of signification in temporal sociocultural processes.[36] Here the focus is meaningful performance as well as underlying competence. Ritual is a transformative performance revealing major classifications, categories, and contradictions of cultural processes. It is not, in essence, as is commonly supposed in Western culture, a prop for social conservatism whose symbols merely condense cherished cultural values, though it may, under certain conditions, take on this role. Rather does it hold the generative source of culture and structure, particularly in its liminal stage. Hence, ritual is by definition associated with social transitions, whereas ceremony is linked with social states and statuses. Ritual symbols, in processual analysis, are regarded as the smallest units of ritual behavior, whether object, activity, relationship, word, gesture, or spatial arrangement in a ritual situation.[37] They are factors in social action, associated with collective ends and means, whether explicitly formulated or not.[38]

Because the analytical frame is processual and embeds meaning in contexts of situation, definitions assigned to terms do not always coincide with those made by linguists and cognitive structuralists. Thus symbol is distinguished from sign both by the multiplicity (multivocality, polysemy) of its signifieds, and by the nature of its signification. In symbols there is always some kind of likeness (metaphoric/metonymic) posited by the framing culture between signifier (symbol-vehicle) and signified(s); in signs there need be no likeness. Signs are almost always organized in "closed" systems, whereas symbols, particularly dominant symbols (which preside over or anchor entire ritual processes), are semantically "open." The meaning is not absolutely fixed, nor is it necessarily the same for everyone who agrees that a particular signifier ("outward form") has symbolic meaning. New signifieds can be added by collective fiat to old signifiers. Moreover, individuals may add personal meaning to a symbol's public meaning, either by utilizing one of its standardized modes of association to bring new concepts within its semantic orbit (metaphorical reconstruction) or by including it within a complex of initially private fantasies. Such private constructions may become part of public hermeneutics or standardized interpretations provided that the semantic manipulator has sufficient power, authority, prestige, or legitimacy (e.g., he may be a shaman, prophet, chief, or priest) to make his interpretation stick. Political symbols have been analyzed in similar terms by A. Cohen,[39] R. Firth,[40] A. Legesse,[41] and V. Turner,[42] among others.

The anthropological study of symbolic forms and processes and the functions of symbolism has generally thrived in the past decade. Where it has been influenced by linguistics or structuralism the stress has been on the eliciting of abstract systems of symbols and meanings from cultural "products" (myths, kinship nomenclatures, iconographic forms, ethnotaxonomies, texts on customs drawn from native informants by questionnaires, etc.). Processualism, on the other hand, demands a kind of fieldwork in which the investigator becomes involved

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with central sociocultural processes. He recognizes his own role in social interaction with his informants and tries to account for the biases this may impart to his subsequent analyses. Symbolic analysis here rests on data generated in the heat of action in ritual, legal, formal, informal, interpersonal, domestic, ludic, solemn, etc., processes to which the anthropologist has become party and privy. Such data are quite different from those obtained by a stance of detachment. This stance is best for the taking of measurements (gardens, hut sizes) or the counting of heads (village census-taking), but worst for coming to an understanding (itself a process) of how actors perceive, generate, and negotiate meaning, using words and symbols. The present author has suggested that there are natural units of sociocultural process, which tend to have, like raw *rites de passage*, a temporal structure, with successive phases cumulating to at least a temporary resolution. The duration, internal structuring, and style of processual units are influenced by biotic, ecological, and cultural variables which must be empirically investigated in each population under survey. Extended case histories may contain a sequence of several processual units of different types, ranging from those which maximize cooperation to those which maximize conflict. Different kinds and intensities of social control functions are brought into play. What is required is a workable cross-cultural typology of processual units. For it is in the analysis of the "social drama"[43] and the "social enterprise"[44] that we recognize the merit of Sally Moore's comment: "An anthropology exclusively focused on clear regularities of form, symbol, and content, and their presumed congruence (whether 'structural,' 'cultural,' or 'processual' in orientation) is leaving out fundamental dimensions. The negotiable part of many real situations lies not only in the imperfect fit between the symbolic or formal level and the level of content, but also in the multiplicity of alternatives and meaning within each, which may accommodate a range of manipulation, interpretation, and choice. Individuals or groups may exaggerate the degree of order or the quality of indeterminacy in their situations for myriad reasons."[45] How they do this, and why, can be ascertained only if the investigator has also become an actor in the field of living relationships. There are risks in not staying aloof, of course, but the acquisition of knowledge has always been beset by dangers, here physical as well as intellectual!

In conclusion, it may be permissible to indulge in a few personal opinions and speculations. Clearly, the great breakthrough or paradigm shift has not yet occurred, if by *paradigm* we mean, with Kuhn, "an accepted example of actual scientific practice—which includes law, theory, application and instrumentation together which provides the models from which spring coherent traditions of scientific research."[46] But there are signs of convergence among hitherto isolated subdisciples, as mentioned above. If these can be united in a single work, either by a single mind, or by a team of interdependent specialists, then the reflexivity among biological, ecological, social, and cultural anthropologists, systemically relating their concepts (and modifying them mutually in the process) in relation to a body of both phenomenologically and empirically generated data, may produce a paradigm comparable in its own way to those from which have sprung coherent traditions of natural science research. But to achieve this goal anthropologists will have to sink certain unimportant structural differences, and processually achieve a relationship of *communitas* with one another, a relational quality of full unmediated communication, even communion, between definite and determinate identities. Such *communitas* would essentially be a liminal phe-

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nomenon, consisting of a blend of humility and comradeship—such as one sees among liminars in the ritual process in simple societies "on the edge of the world."

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GORDON R. WILLEY

A Consideration of Archaeology

IN THE LAST DECADE OR TWO archaeology has undergone a very rapid transformation. What has been a rather quiet contemplation of the past, a kind of savoring of its substance not always untinged with a little romanticism, is now being brought forward into the more strident world of "relevance." The past—the vast preliterate past as well as the more immediate historic past—is to be made germane to the present. Of course, in one sense this linkage of past and present has always been the mainspring of archaeology. The archaeologist deals with life, or its residues, and this subject matter can be interpreted only in terms of the living. What is new are theoretical attitudes about the data and methodological procedures for handling them. These attitudes and procedures combine to compose what is often referred to as the "new archaeology." This body of theory and method may be said to be truly and uniquely archaeological. In this there can be no denial that various techniques in such things as dating and materials analyses—borrowed from the physical, chemical, and natural sciences—have had an important effect in the development of such a new archaeology, but such ancillary methods and techniques are not, in and of themselves, at the core of the new outlook.

This new outlook, rather, consists of a number of conceptual features. Even though some of these are not entirely new, it is fair to say that the emphasis upon them and their formal methodological structuring are innovations. One of these features is the principle that all societies and cultures, past and present, were or are systemically organized and that the functionings of their subsystemic parts and the trajectories of development of these parts and of the whole are to be understood with the aid of general systems theory. Closely and immediately linked to this systemic view is the very basic archaeological principle already referred to that the remains and residues of the past are to be explicated by means of analogies with life situations. Relating to all of this is the concern with process and the conviction that "laws" or "regularities" governing the processes of social and cultural change are, indeed, discoverable and that this, rather than the narration of history, or prehistory, is archaeology's principal goal—a goal that leads to "relevance." This body of theory is to be made operational by the testing of hypotheses about the applicability of analogical models, the workings of culture systems, and the validity of laws or regularities through strategically devised archaeological exploration and analysis. As can be seen, the objectives are ambitious and optimistic.

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Although the new archaeology has had its most recent and immediate origins in the United States, and has probably taken its purest form here, its development should be viewed in wider perspective. Although archaeology had its beginnings in Renaissance humanism and romantic antiquarianism, it began to diverge in the nineteenth century into two branches,[1] one of which continued essentially in the humanistic tradition. This was the archaeology of the Classic World and the Near East which has come down to the present in the academic houses of Classics and Art History. The other branch, with which we are primarily concerned, moved off in the direction of the natural sciences and of anthropology. It developed most quickly in Denmark, England, and France, where its subject matter was largely that of preliterate cultures. In Europe this kind of archaeology became known as prehistory. In the United States it grew up in departments of Anthropology. Here the ethnologist and the archaeologist were both dedicated to the study of the American Indian, living or dead, so that the alliance was a logical and practical one.[2] For a good while the problems of American archaeology, as was also true of Old World prehistory, were those of description, taxonomy, and chronology; but some of the most imaginative research derived from the connections with ethnology.[3] These were the linking of prehistoric cultures with historic or modern Indian tribes, as in A. V. Kidder's work in the Southwestern Pueblo Indian area or in W. D. Strong's investigations on the North American Great Plains.[4]

In the 1920s and 1930s some important theoretical innovations appeared in European prehistory. V. Gordon Childe, in a series of important books, began to move away from the limited chronology building that had been the goal of traditional prehistorians and to define and map archaeological cultures and societies.[5] Grahame Clark continued in this direction with the reconstruction of prehistoric environments and the development of the ecosystem concept in British archaeology. In a book published in 1940, called *Archaeology and Society*, Clark took a systemic view of culture and society and attempted to say something about the interrelationships of environment, technology, social forms, and idea systems.[6] In so doing, he was trying to synthesize some of the ideas of the functionalist social anthropologists of the British School with the "earth sciences" aspects of archaeology that were being developed by means of soil studies and pollen analyses.

In the United States at this time, in spite of the traditional academic union of archaeology with ethnology and social anthropology, such new departures were much less definite, although W. R. Wedel, working on the Great Plains, initiated archaeological-environmental studies,[7] and Paul S. Martin and his associates tentatively advanced some interpretations of prehistoric Southwestern Pueblo social organization from analyses of architectural features and artifacts.[8] More interesting from a theoretical standpoint were some strictures on the unimaginative nature of American archaeological research that were first set forth by American ethnologists[9] and later developed into a book, *A Study of Archaeology*, by the archaeologist Walter W. Taylor. In this latter work Taylor issued a call for a greater concern by archaeologists with contextual control, function, and process.[10]

The Second World War intervened and stopped archaeological research for

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a time. Afterward Clark continued along his innovative lines. In the Americas Taylor's book was met with hostility or silence—or so it seemed at the time. Yet, in retrospect, it must be remembered that we are talking of very short time spans in the development of any discipline, and as we look back over the published record it is obvious that some ferment was going on in American archaeology. During the late 1940s and 1950s an increasing number of archaeologists were motivated by problems other than simple descriptive-chronological ones. Good examples of this in the Americas would be the research that was conducted into the processes of the rise of native agriculture in Mexico[11] or the study of Peruvian prehistoric settlement patterns as a basis for making inferences about population sizes and groupings and their social and political interrelationships.[12] The results of these investigations were presented substantively, with little theoretical or methodological emphasis, or polemical accompaniment, but they clearly owed something to the kinds of questions that Childe, Clark, and Taylor had been posing. All of this—a seeding of new ideas, their rejection or apparent rejection on the part of some, their acceptance, however covert or even subconscious, on the part of others—climaxed in the early 1960s with the new archaeology.[13]

The promulgation of the new doctrines has been accomplished with a certain amount of revolutionary fervor, now somewhat abated, but it is fair to say that these doctrines have gone a long way toward capturing the field, at least here in the United States. For the most part, there has been relatively little articulate opposition to the new archaeology within the American archaeological fraternity.[14] To some extent, this has been because a good many older practitioners have felt they were on to much of it before the "Young Turks" raised the banner and have stood aside, somewhat aggrieved, only because this fact did not seem to be recognized. More basically, this acceptance has a good deal to do with American archaeology's long association with ethnology, social anthropology, and the social sciences in general. Although there are countercurrents in these fields, their general tone has always been positivistic and, above all, optimistic. There is a general consensus that problems are susceptible to solution in a scientific manner, by means of quantitative treatments and in other pragmatic ways. In a word, they are imbued with the very American notion that intelligence and diligence can overcome the greatest obstacles, and the current optimism of archaeology is very consonant with all this. And, in this connection, it is probably no accident that the strongest criticism of the new orientation has come from the humanistic tradition in archaeology, and especially from those British archaeologists whose views are not particularly compatible with those of Childe or Clark, and for whom, in many cases, "social science" is a "dirty word."[15]

This separation between anthropological and humanistic archaeology has never been more clearly emphasized than it is now. As we have seen, the separation is as old as the profession itself, but for a long time it was reflected mainly in the choice of subject matter. Now it has been underlined as a basic difference in theoretical orientation and goals. The crux of the matter is the degree to which material aspects of culture do articulate systemically with nonmaterial ones and the possibilities for detecting these articulations in the

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archaeological record alone. The issues have been stated by M. I. Finley, a leading classical archaeologist, in a recent number of this journal.[16] Finley begins by quoting L. R. Binford, who has written that

> The argument that archaeologists must limit their knowledge to features of material culture is open to serious question; and second, the dichotomy between material and nonmaterial aspects of culture itself and the relevance of this dichotomy for a proposed hierarchy of reliability have also been the subject of critical discussion. . . . It is virtually impossible to imagine that any given cultural item functioned in a socio-cultural system independent of the operation of "non-material" variables. Every item has its history within a socio-cultural system—its phases of procurement of raw material, manufacture, use, and final discarding. . . . *There is every reason to expect* that the empirical properties of artifacts and their arrangement in the archaeological record will exhibit attributes which can inform on different phases of the artifact's life history" [Finley's italics].[17]

Finley then goes on to ask:

> Is there any reason to expect what Binford expects, and significantly can offer only as an expectation rather than as a proposition for which there is available evidence? On the contrary, there is sufficient evidence that identical artifacts and arrangements of artifacts can result from different socioeconomic arrangements of procurement, manufacture, or distribution. For example, we know from the chance preservation of accounts inscribed on stone, that the most delicate stone carving on the temple in Athens known as the Erechtheum was produced by free men and slaves working side by side at the end of the fifth century B.C. Nothing in the material remains (the carving itself) could have told us that. On the other hand, the surviving accounts of the temple of Apollo at Epidaurus, built thirty or forty years later, are of such a nature that the labor force is not specified. How does Binford imagine it will be possible to discover whether or not slaves were employed, at the highest skill level, on that temple?[18]

Admittedly, many telling arguments like this can be made against the optimism of the new archaeology, and there are aspects of the past, both nonmaterial and material, that archaeology will never recover; yet I find myself sympathetic to the optimists in this case. Perhaps there is nothing in the Erechtheum "carving itself" that would offer a clue to the social status of the craftmen who made it; but I cannot help but wonder that if Greek archaeology had been more widely based, in being concerned with such things as humble settlement quarters and craftsmen's tools and their contexts, there might have been some high probability answers to questions like this about 5th century B.C. society. This is not necessarily a criticism of Greek archaeology—about which I know very little—but I know that in my own field of Maya archaeology the past hundred years or so of research has been directed mostly toward the grand and the spectacular, the palace and the temple rather than the hut. There was nothing wrong in beginning this way; indeed, it would have been rather peculiar to have initiated investigations at the humble end of the scale; but temples did exist in larger socioeconomic contexts or systems, and many of the things that concerned them will almost surely be explained by what went on in other parts of those systems. It is certainly too soon to close the door on such possibilities. And it is also too soon to be overly pessimistic about a working reconciliation between anthropological and humanistic archaeology.

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The relationships of material to nonmaterial aspects of culture involve the uses of analogy as well as systems theory, and this constitutes another debatable area between anthropological archaeology, as represented by the new archaeology, and more traditional archaeology, especially of the humanistic brand. Insofar as the source materials are concerned, there are essentially three types of analogy open to the archaeologist. He can work through *specific historical analogy*, which is usually what is meant by the term "ethnographic analogy." This refers to situations where the archaeological culture is interpreted by means of analogies drawn with ethnographically or historically known cultures of the same general geographic region and the same general culture-historical tradition. The assumption is that the prehistoric culture is related, in an antecedent sense, to the historic or modern one. There are many examples of this, especially in the Americas, including those already cited for the Southwestern United States Puebloan area or the North American Great Plains, where a reasonable case for continuity has been made through combined archaeological and ethnohistoric research. The prehistoric-to-historic sequences of Mexico and Peru are other obvious examples. Here the descriptions of Aztec and Inca civilizations, as provided by the sixteenth-century Spanish conquistadores, have given a baseline for interpreting the earlier cultures of those areas.

Clearly, there are limitations and disadvantages to such specific historical analogies. Continuities from prehistoric past to historic present are not always easy to demonstrate, and even when this can be done it must be kept in mind that formal material similarities do not necessarily imply continuities in nonmaterial aspects of culture. Specific historical analogy has not been scorned by contemporary American archaeologists. J. J. F. Deetz's studies on changes in Arikara social organization, through the tracing of changes in ceramic decorative attributes and their associations with house site foundations, worked from historically known ethnographic data about Arikara kinship and residential patterns[19]; and W. A. Longacre's somewhat similar studies in Southwestern Pueblo ruins also took off from ethnographic information about the Western Pueblo Indians of the historic and modern periods.[20] Nevertheless, the new archaeological position on specific historical analogy is not an altogether favorable one.

Binford has addressed himself to this on several occasions, and what he objects to, in addition to those disadvantages already mentioned, is that specific historical analogy, or any kind of specific ethnographic analogy, limits the possibilities of developing an "independent" archaeological methodology. His views are well summed up in the following quotation:

> . . . such a procedure [the use of ethnographic analogy] denies to archaeology the possibility of dealing with forms of cultural adaptation outside the range of variation known ethnographically. . . . In view of the high probability that cultural forms existed in the past for which we have no ethnographic examples, reconstruction of the lifeways of such sociocultural systems demands rigorous testing of deductively drawn hypotheses against independent sets of data.[21]

Although I would agree with Binford "that cultural forms existed in the past for which we have no ethnographic examples," I can see no other way in which past behavior may be inferred except with reference to known living behaviors.

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Thus, even though it is true that there is probably no currently known or historically recorded living pattern in the world that is a good analogue for that represented by an Upper Paleolithic cave site in France, the debris and artifacts from that site can be placed in a simulated life situation only by our general knowledge and awareness of hunting peoples—that they pursue and kill game, make stone tools for doing this, and adjust to seasonal rounds in the food quest. This kind of analogy, *general comparative analogy*, is based not on specific historical or ethnographic situations but on our general knowledge of life itself. In many instances it is the archaeologist's only interpretive recourse. It does not, however, free the archaeologist from some kind of known, living referents, even if these are drawn by him from disparate sources and assembled into a model of his own making.

In this connection, I think it should be said that general comparative analogy has very definite limitations and even grave risks. In constructing a life model from such "bits and pieces," randomly drawn from space, time, and cultural tradition, the design of the model must be either very "loose-jointed" or else one that stands in very great danger of being influenced by personal or doctrinal predispositions.[22] I would second the desire, on the part of some archaeologists, to make archaeology a "science of material culture,"[23] for in the last analysis this is where its methodological nexus is located. We must interpret through material remains. But we cannot be sure, except through doctrinal insistence, that this is where the root of all process lies. As we interpret through analogy, in an attempt to lay a base for a "science of material culture," I think that the source of the analogical model may be more important than some archaeologists have admitted. Binford and others have insisted that it is not the source of the model but its testing that is crucial. The importance of testing cannot be denied; but archaeological data do have a way of being made to verify initial hypotheses and models. I think that these attitudes about general comparative analogy, or "independent archaeology," are at the base of much of the humanistic objection to the new archaeology.

There is, in this regard, a middle ground in analogy which operates with real life models and which may be more compatible with humanistic tastes than the general comparative pastiche. This could be called *specific comparative analogy*. It frees the archaeologist from some of the limitations of specific historical analogy but remains more within the bounds of human experience than the general comparative approach.[24] This is where the model is drawn from a specific culture and society, known ethnographically or historically. The model is applied, however, to the interpretation of an archaeological culture to which it has no historical connection. Such a procedure is hardly new. Philosophers of history are old hands at it, although the analogies drawn by Spengler or Toynbee were not primarily for the purposes of archaeological interpretation. But in recent years this kind of comparative analogical model has been widely used by archaeologists. One of the most interesting examples of this in American archaeology concerns the Olmec civilization of ancient Mexico and Central America.

The Olmec civilization appears to have been the first "advanced" culture of Precolumbian Mesoamerica. Its most impressive sites are located in the Gulf Coast lowlands of the Mexican states of Veracruz and Tabasco, and these sites, which date to the period of ca. 1200–900 B.C., are the earliest in Mesoamerica to

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feature such things as large pyramids and platforms and monumental stone sculpture. The sculptures are in a very sophisticated and distinctive art style, and this style is replicated in smaller works of art, such as jade or pottery figurines or other manufactures. At this period of Olmec florescence the other regions and cultures of Mesoamerica were on a much simpler level of cultural development; however, in many of these other regions, from Central Mexico south to Salvador, Olmec art and iconography was manifest, both monumentally and on smaller, portable items, in what were otherwise more modest cultural contexts. This presented archaeologists with the interesting questions as to just what this signified, how was it disseminated, and what was the meaning of this widespread Olmec influence?

Both general comparative and specific historical analogies were addressed to the problem. Political imperialism, accompanied by religious proselytization, radiating from Olmec Gulf Coast centers is one model of the first kind.[25] A specific historical model was advanced by M. D. Coe[26] in which Coe suggested that Olmec hegemony over other parts of Mesoamerica was effected and maintained by a combination of military and economic dominance comparable to that known to have been used by the Aztecs in the same general area some two and a half millennia later. According to this model, the Aztec *pochteca*, a professional, state-supported cadre of merchants, established an extractive trading network throughout the area, channeling raw materials and other goods into Tenochtitlan, the Aztec capital, and these merchants were backed up by an aggressive military aristocracy. Coe's *pochteca* model had the specific historical advantage of pertaining to the area and cultural tradition to which the Olmec belonged, and some rather tenuous continuity can be demonstrated between Olmec and Aztec cultures; but the 2500 years separating the two is a long time, and, for various reasons, the Aztec model seemed unsatisfactory to a good many other archaeologists as an explanation of what had gone on in Olmec times. As an alternative to either the very general "imperial" model, or the *pochteca* model, K. V. Flannery has proposed a specific comparative model.[27]

Flannery cites two sources for his specific comparative analogical model, ethnographic accounts from two different parts of the world both remote from any connection with Precolumbian Mesoamerica. One of these is in Burma and concerns the Shan and Kachin tribes. The Shan were valley-dwelling, wet-rice cultivators with a stratified society. The Kachin lived in the neighboring hill country, practiced a less productive slash-and-burn form of agriculture, and had either an egalitarian or a ranked lineage social order. The Kachin, however, were in control of country rich in certain mineral resources, including gold and jade, both of which were lacking in the Shan-held lowlands. The Shan aristocracy desired these precious materials for the making of prestige ornaments or for trade with the Chinese. To obtain them they not only traveled to the hill country but established kin ties with the more primitive Kachin by giving their daughters in marriage to the Kachin chiefs. Such alliances worked to their advantage in obtaining the minerals, and they also exported foodstuffs, from their surplus supplies, to the hills. As a result of these contacts certain Kachin leaders began to imitate Shan nobility in ritual and religion, including the possession of symbolic objects of Shan design, and they also began to imitate features of the Shan stratified social organization.[28]

The other processual model is that offered by the Tlingit, of the Northwest

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Coast of North America, in their relationships with the Tagish. The former were wealthy salmon fishers with an aristocratic leadership, the latter poorer and socially simpler inland hunters. But, again, the poorer tribes controlled resources that were particularly prized as status items and as trade goods. In this instance it was the Tagish control of furs. As in the Shan-Kachin example, Tlingit aristocrats came to the Tagish country, took wives there, and sent daughters there to marry, securing valuable trading rights in doing so. Through these contacts the Tagish became "Tlingitized" to a degree, borrowing rituals and becoming more conscious of social rank in their own society.[29] Neither the Kachin nor the Tagish, in their respective situations, underwent any basic subsistence, technological, settlement pattern, or fundamental ethnolinguistic change; yet in both Burma and the Northwest Coast processes were set in motion which, if continued, could have resulted in an increasingly tighter area interaction sphere, pertaining to trade, sociopolitical organization, and probably ideology, as it did in Precolumbian Mesoamerica.

Flannery has used these models in an attempt to explain the Olmec "presence" in archaeological sites in Oaxaca that were built and occupied in the early part of the first millennium B.C. By this time population groups in the Valley of Oaxaca had reached a size and density where some degree of sociopolitical complexity had developed. This is seen in corporate labor constructions, such as platform mounds above that of ordinary residential dwelling mounds; and this and other developments, including signs of a redistributive economy, signal an emerging Oaxacan elite of a chiefdom level. From all evidences, however, this early Oaxacan elite was distinctly less sophisticated than the Olmec aristocracy of the neighboring Mexican Gulf Coast country. At the same time, though, these Oaxacan leaders apparently controlled local resources of minerals, particularly magnetite and ilmenite, which were used in the Olmec country to manufacture ritual mirrors. Referring to the Oaxacan situation and to the specific analogies, Flannery states: "If our ethnographic data are in any way analogous, the mechanism which facilitated this inter-regional flow of goods should have been one which linked the highest ranking lineages of the Oaxacan peoples to one or more of the higher-ranking Olmec lineages. One would also predict that the highland Oaxacan elite would begin to emulate the religion, symbolism, dress, and behavior of the Olmec elite, insofar as it would enhance their own status among their own people. We might predict, for example, that while their patterns of settlement and subsistence remained unchanged, they might adopt the St. Andrew's cross, the U-motif, the 'paw-wing motif,' and the deity who was part man and part jaguar [all elements of Olmec iconography—GRW]."[30]

Many of the Oaxacan data do, indeed, confirm these predictions and suggest that the Burmese and Northwest Coast models may provide apt parallels for understanding the spread of Olmec influence in ancient Mesoamerica. Flannery did not offer it as any conclusive explanation of the Olmec phenomena. Such analogies are trial formulations, and the devising of "tests" to validate such models of process are frequently difficult. Flannery's use of specific comparative analogy in this instance is appealing because it moves closer to the specifics of behavior than do the more general models, which simply propose such concepts as "empire" or "trade." It also deals with analogies that are drawn from

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preindustrial societies of a developmental level at least somewhat comparable to the societies of ancient Mesoamerica. To be sure, some Mesoamericanists have criticized Flannery's model. From my own point of view, I doubt if food exchange, which figured importantly in the Shan dealings with the Kachin, is probable over the long distance separating Oaxaca and the Olmec Gulf Coast. But this and other substantive matters can be worked out only by continued investigations. The use of specific comparative analogical models is a first step.

A great many other examples of specific comparative analogy could be cited. It is more and more a standard operating procedure in archaeology. Not the least of its merits is that it forces archaeologists to be less immersed in their own narrow interests. When they return to their particular bailiwick, after a comparative look at cultures—ethnographic, historic, or archaeologic—unrelated to their own immediate research concerns, they are often able to look at what has grown familiar to them with a new and discerning eye.[31] One direction in which the comparative analogical approach can lead is toward what might be called a typology of cultures and of process. This, of course, takes us back in the direction of the philosophers of history; but some archaeologists have been doing this on a more limited and more fine-grained scale than that essayed by Spengler or Toynbee. Robert M. Adams has examined Mesopotamian data on the development of such institutions as land tenure and its relationships to the growth of the state, and has compared this story of process with one for ancient Central Mexico culminating in the Aztec empire.[32] His study is of great interest and holds promise for an understanding of "regularities" in process. Colin Renfrew, drawing widely upon European archaeological and historical data, as well as upon Polynesian ethnography, has described what he calls "Group-Oriented" and "Individualizing" chiefdoms.[33] These can be thought of as culture types, identifiable by specified archaeological traits, but they are of greater value in explicating processes of development in the general evolutionary stage between egalitarian societies and those that can be considered to be on the level of the state. All of this leads toward the concern with laws or regularities; but before facing up to this most difficult of all archaeological topics, let us review, however briefly, something more of the ambience in which the new archaeology has developed in recent years.

I have already stated that the theoretical or conceptual heart of the new archaeology is uniquely archaeological, and this is so. At the same time, its development has been greatly stimulated and forwarded by the deployment of methods and techniques from other sciences. As Grahame Clark has stated recently, archaeology is now science based. But, as he goes on to explain: "In the study of prehistory there is a constant interaction between aims and methods."[34] Thus, the desire on the part of archaeologists to know more about prehistoric diets has undoubtedly been a stimulus which they have taken to the soil scientists and paleobotanists; and the need to know more about ancient trade patterns has led archaeologists to inspire physical and chemical scientists to explore trace element analyses of obsidian or to determine the composition of metals. And all of this has had a feedback effect in leading archaeologists to ask new and more difficult questions of their data as these new methods and techniques have been continuously improved and refined. I shall make no attempt here to enumerate or describe all of the aids that now come to

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archaeology from other fields. In addition to the soil, pollen, and materials analyses just mentioned, the most important ones are dating techniques, especially through radiocarbon, and mathematical applications. In this last connection the computer has made it possible for archaeologists to ask questions about prehistoric demography, social organization, and land utilization that, heretofore, have been considered beyond the scope of the discipline. Indeed, general systems theory in archaeology, one of the primary theoretical underpinnings of the new trends, rests most solidly on mathematical simulations of systems models.

To this point I have referred to systems theory only in a general way in connection with the proposition that cultures are systems in which their parts, nonmaterial as well as material, relate to each other. Much recent archaeological writing has been devoted to systems theory, in both general[35] and substantive contexts. Among the latter are studies on subsistence changes, factors governing settlement patterns, Post-Pleistocene ecological adaptations, and the rise and fall of civilizations.[36] And this is only the smallest sort of sampling as one looks over current archaeological literature. There can be no doubt that the approach has a great potential for archaeology. In a sense, the idea has always been present, in a covert way, in archaeology; but, as used now, the clear delineation of the elements of the cultural system, and an explication of the interaction of these elements, give a greater openness and objectivity to archaeological interpretations and the reasoning behind them.

However, it should be realized that the construction of a simulation model of a system and the demonstration of the internal consistency of that model do not necessarily offer proof of what actually happened in the past. Such a model, for example, has been constructed in an attempt to explain what brought about the "collapse" of Classic Maya civilization in the lowlands of southern Mexico and Central America at the end of the ninth century A.D.[37] The various factors introduced into the model—population, resource pressures, foreign pressures, trade, prestige building activities, the exploitation of commoners by the nobility, and so on—are all reasonably documented in the archaeological record, and a convincing case can be made out for their positive or negative feedback interactions on each other in a way that seems to "explain" the denouement and breakdown of the Maya. It is the best that we have been able to do so far, yet I have a feeling that its main value will lie in its detailing of an analytical procedure that attempts to take account of as much of the data as possible and that it will probably be superseded as we come to know more.

In addition to the infusion of ideas and analytical methods from the mathematical, physical, chemical, and natural sciences, the new archaeology has been greatly encouraged in the pursuit of its objectives by the vast increase in archaeological research since the Second World War. Prior to this time there were still large areas of the world for which prehistory was still an unknown quantity. Sub-Saharan Africa, Siberia, much of Southeast Asia, and huge stretches of North and South America were among these. Now something is known of all of these places, and for parts of them there is detailed information. For other areas, previously partially or sketchily known, research has filled in geographical and chronological gaps and has generally refined our knowledge of cultural distributions and chronologies. This, together with radiocarbon dating

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scales, has given archaeologists a control over their data never enjoyed before. They are ready now, for the first time in many places, to ask questions about such things as the relative rates of culture change in different sociocultural situations or to appraise the processes of in situ development vis-à-vis those of diffusion. For the Old World we know now that settled community life, based on plant and animal domestication, had its beginnings as far back as 9000 B.C. and that after this, in the Middle East, there was a generally slow evolution leading up to the threshold of urban life at about 3000 B.C.[38] For the New World we can trace a similar evolution but the datings are later. In Mesoamerica and in Peru some plant domestication was taking place in the millennia between 7000 and 2000 B.C., but only toward the end of this period did the societies of these areas attain what has been referred to as a "village agricultural threshold."

After this, however, the New World evolution moved more swiftly, climaxing in true city life at the beginning of the Christian era.[39] This difference in timing is interesting and one of the many things in prehistory that is unexplained. In both the Old and New Worlds archaeologists are just beginning to learn something about the role of tropical forest areas in the rise of agriculture. Previously, attention had been directed at upland or semiarid zones in both hemispheres and to seed crops (wheat, barley, millet for the Old World; maize for the New); but in the last few years evidence has come in which would indicate that centers in Southeast Asia[40] and Tropical Forest South America[41] had developed root crop cultivation at very early times and that these centers had interacted with neighboring upland or nonforested regions, in their respective settings, to influence and augment the rise of a farming economy. This is not the place to continue with a recounting of substantive discoveries and advances in prehistory. The point to be made is that we know much more in this way than archaeologists of a quarter of a century ago, and although no one archaeologist can be said to have mastered the vast field of world prehistory, almost all of them have a greater awareness of it than was previously the case. This has made for a comparative outlook, as has been said with reference to analogical models, and has provided an atmosphere sympathetic to an interest in process.

It should be noted, of course, that much of the archaeological research that we have been talking about was carried on without any particular new archaeological emphasis, and this raises questions about research philosophy and procedures. Should a taxonomic-space-time framework, an archaeological data base, be laid down before the archaeologist attempts to move on to problems of process? Binford has argued that it cannot, that the asking of questions about process must be the beginning of any field investigation, for only if these problems are well in mind can the investigator devise the proper research strategies and to know what kind of evidence he is seeking.[42] There is something to be said for this position, but it is equally true that to ask the proper questions demands a certain foreknowledge of the prehistory of the region under examination. Fortunately, there is at least a practical answer. Any modern archaeologist should have the kind of awareness and vision that combines a data base recovery with a concern for process.

If there is some justification for the optimism that a systemic view of society and culture will take us further than we have ever gone before in explaining

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man's past—and I think that there is—and if this can be expedited through judicious selection of analogical models and their deductive testing, is there also a justified optimism about the discovery of laws or regularities concerning process? The humanistic branch of archaeology, and probably most historians, would say no. V. Gordon Childe, the foremost new archaeologist of the pre-new archaeological era, also concluded in the negative. According to Grahame Clark ". . . in 1946 he [Childe] could still argue that there was a 'prospect of reaching general laws indicative of the direction of historic progress,' in his 'Valediction' he had to admit that while Marxism had once seemed to make intelligible the development of each culture it 'completely failed to explain the differences between one culture and another and indeed obliterated or dismissed the differences observed' (1958:6)."[43]

Binford, on the other hand, takes a more positive view.[44] To him, the *explanation* of cultural differences and similarities is evolutionary study, and this is the road to the formulation of laws. In saying this he rejects J. H. Steward's[45] "neo-evolutionism" as not evolutionary or processual but, rather, a "descriptive concern with . . . patterning," the comparing and generalizing about the changes through time that are observed in several culture sequences. Binford goes on to argue that in true evolutionary study the unit must be the whole independent sociocultural system. In his definition, evolutionary process is an operation at the interface of a living system and its extrasystemic field. Presumably, this "extrasystemic field" would be not only the natural environmental or ecological interface but the interfaces created by contact or communication with other sociocultural systems, although this is not altogether clear. He is, however, very explicit in stating that attempts "to use units which are partitive or units which represent classes of cultural phenomena will necessitate the added investigation of intersystemic relationships." It is here that difficulties arise for one "class of cultural phenomena" is certainly ideology, and it is to the realm of ideology and ideas that evolutionary theory often seems least applicable.

Does ideology articulate with other aspects of culture in a systemic fashion? Ethnologists and social anthropologists have been exploring this lately[46]; and Flannery, together with Joyce Marcus, has followed their lead by trying to see if these articulations can be discerned on the prehistoric level by archaeologists.[47] In this attempt, directed toward prehistoric Oaxacan cultures, they have relied, to a considerable degree, on some specific historical analogies in order to project ideological meanings (as derived from the Spanish conquest period in Mexico) back onto the first millennium B.C. Their analogical models reveal a Zapotecan (Oaxacan) cosmology (world view or idea system) that was intimately linked with agriculture, trade, ceremonialism, death and ancestor worship, and class stratification, and from the archaeological disposition of artifacts bearing specific iconographic symbols or otherwise identifiable as ritual paraphernalia they are able to plot a course of development through time in which ideology is seen as an integral part of the evolution of a cultural system. Their concluding paragraph might be taken as a directive for future archaeological research:

> In our opinion two of the most common mistakes being made by students of prehistoric human ecology are the attribution of Western economic motives to pre-

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> Columbian subsistence behavior and the dismissal of pre-Columbian ritual as a form of intellectual activity unrelated to subsistence. Neither approach is supported by an examination of Zapotec cosmology. The Zapotec world was an orderly place in which human actions were based on empirical observations, interpreted in the light of a coherent body of logic. Once that logic is understood, all Zapotec behavior—whether economic, political, or religious—makes sense as a series of related and internally consistent responses based on the same set of underlying principles. In other words, one very non-Western metaphysic regulated exchanges of matter, energy, and information. We raise these criticisms not to detract from the study of human ecology but to make it still more holistic and hence more useful. Our goal is to find a framework for analysis which is neither a mindless ecology nor a glorification of mind divorced from the land.[48]

This quotation, and especially its last sentence, brings us to our most serious question about the new archaeology, its practice to date, and, more generally, the goal of process. Is it all sufficiently encompassing? So far, in my opinion, it has not been; but, admittedly, this kind of an outlook is still very new. The data of archaeology are, by definition, in the material realm, but the goals of the discipline must transcend this limitation. The processes of culture change must be studied not only in the ecological interface but at all of those points where ideas or "cultural choices," insofar as these can be inferred, intersect with economy, technology, and the more immediately materialistic aspects of culture. Ultimately, archaeology will not profit from a further breaking down of questions about human behavior and motivations. A framework for an integrated analysis must be sought. It is not overoptimistic to commit ourselves to such a search, for I think that it is inevitable that the anthropological and humanistic branches of archaeology will at last converge.[49] I doubt if this will come about through any formal academic merger, and it should not. Each tradition has always been a demanding one, and each brings to the study of the human past its own biases which, if not followed blindly, have their appropriate strengths. The convergence should come about in the face of questions about the past, especially the "big" questions. Are there laws or regularities about social and cultural behavior and change? What are the causative forces that make these laws or regularities operative? And what is their relevance to the present, if any? The Marxist paradigm is too narrowly conceived to allow us to discover the answers to questions like these. When we do, it will be with an outlook that enables us to consider the whole range of the human adventure, past and present.

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D. P. MCKENZIE

Plate Tectonics and Its Relationship to the Evolution of Ideas in the Geological Sciences

Introduction

IN 1967 AND 1968 FOUR PAPERS were published (see Cox[1]) which described a collection of ideas now generally known as the theory of plate tectonics. Most of the ideas were not new, but they had not previously been connected into a comprehensive theory of the Earth's surface deformation. Since 1967 plate tectonics has been generally accepted and applied to all of the major divisions of the earth sciences, whereas before that date the ideas produced fierce controversy whenever they were discussed in public. Though some additions have been made in the last ten years no major modifications have been needed. The history of the ideas involved in this revolution has been described in some detail by Hallam[2] and will certainly be a fruitful field for those interested in the history of science. But to me a more interesting, though certainly a more speculative question is why the date is 1967 and not at least fifty years earlier.

The concepts and mathematics involved were familiar to the great geophysicists such as Kelvin, Darwin, Lamb, and Love, of the nineteenth and early twentieth centuries. Though little was then known about the mechanical behaviour of the Earth's interior, a number of key geophysical observations had already been correctly interpreted. Yet nothing resembling the modern theory was put forward before 1965, and its implications even then were not realized for another two years. This failure has been traditionally blamed on the opposition of geologists and geophysicists to the idea of large horizontal displacements. Though there is no question that such opposition did exist, it still existed in 1967 yet had little influence on the rapid acceptance of the new ideas. Furthermore, several of the best known and internationally respected earth scientists, such as Argand, Du Toit, and Holmes, were convinced for many years that large horizontal motions of thousands of kilometres had occurred at the Earth's surface, yet failed either to convince most of their colleagues or to produce the detailed predictive theory we now have. Neither prejudice nor lack of imagination is, I think, a reasonable explanation for these failures. I believe, and will attempt to demonstrate here, that they resulted from a more fundamental cause than has yet been generally recognized: the difference in the behaviour of continental and oceanic rocks when subject to deformation.

But before any historical questions can be examined we need an outline of the theory as it now exists, which is contained in the next section. This is

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followed by accounts of three periods when major discoveries were made which might have led to the emergence of plate tectonics before the 1960s and my own speculations as to why this did not happen. This is followed by a brief history of the events which preceded the theory's general acceptance in the late 1960s, in which I was personally involved. The next section consists of a comparison with the events in biology which occurred in the previous decade. This is instructive as much because of the differences as for the similarities. Finally, it seems appropriate to speculate on the directions which future research in the earth sciences will take, even though the earlier sections suggest that I will achieve little by so doing!

2. *Plate Tectonics*

What distinguishes plate tectonics from earlier descriptions of the Earth's tectonics is the extreme simplicity of its basic postulates. Figure 1 shows the locations of earthquakes in the North Atlantic and Mediterranean areas. In the Atlantic the locations are restricted to a narrow band which lies along the crest of the Mid-Atlantic ridge, and to a zone which extends from the Azores to Gibraltar. The pattern in other oceanic regions is similar: the shocks are restricted to linear belts whose width is comparable to the accuracy with which the earthquakes can be located. This observation suggests that most of the deformation of the surface part of the Earth occurs along narrow linear structures called *plate boundaries*, and that the regions in between, which are called *plates*, are not deformed. This is the essential idea of plate tectonics. The rigid regions are in fact spherical caps, but the consequences of the rigidity are easier to understand on a plane rather than on a spherical earth. Figure 2 shows two flat plates A and B in relative motion. Since there is no way of defining an absolute frame of reference to which we can fix ourselves to measure the absolute motion of any plate, the whole theory must be constructed in terms of the relative motion between two plates. A statement such as "America is moving westward" is meaningless unless an absolute frame of reference can be defined: it is possible to say only that "America is moving westward relative to Eurasia."

In Figure 2 the motion of A relative to B taken as fixed is shown by the solid arrow. There is no reason why B rather than A is taken to be fixed, but it is generally easier to understand plate tectonics if one or the other plate is used as a reference frame. Since A is a rigid plate, the motion of all points relative to B must be in the same direction as the heavy arrow. On the boundary marked by a double line, A is moving away from B. This is possible only if new plate is being created along this boundary, and such boundaries are known as *ridges*. They form shallow linear structures crossing major ocean basins. A surprising feature of ridges is that they add to each plate at the same rate. The reason for this behaviour is not properly understood.

On 2 and 4 the motion of A is parallel to the trend of the boundary. Such boundaries are lines of pure slip and are known as *transform faults*. They are important because they show the direction of relative motion and are often easy to recognize. Those in Figure 2 join a ridge to a trench, but they can also join two ridge segments or two trench segments (Wilson[3]). Boundary 3 is a boundary along which one plate moves under the other and is consumed. Such bound-

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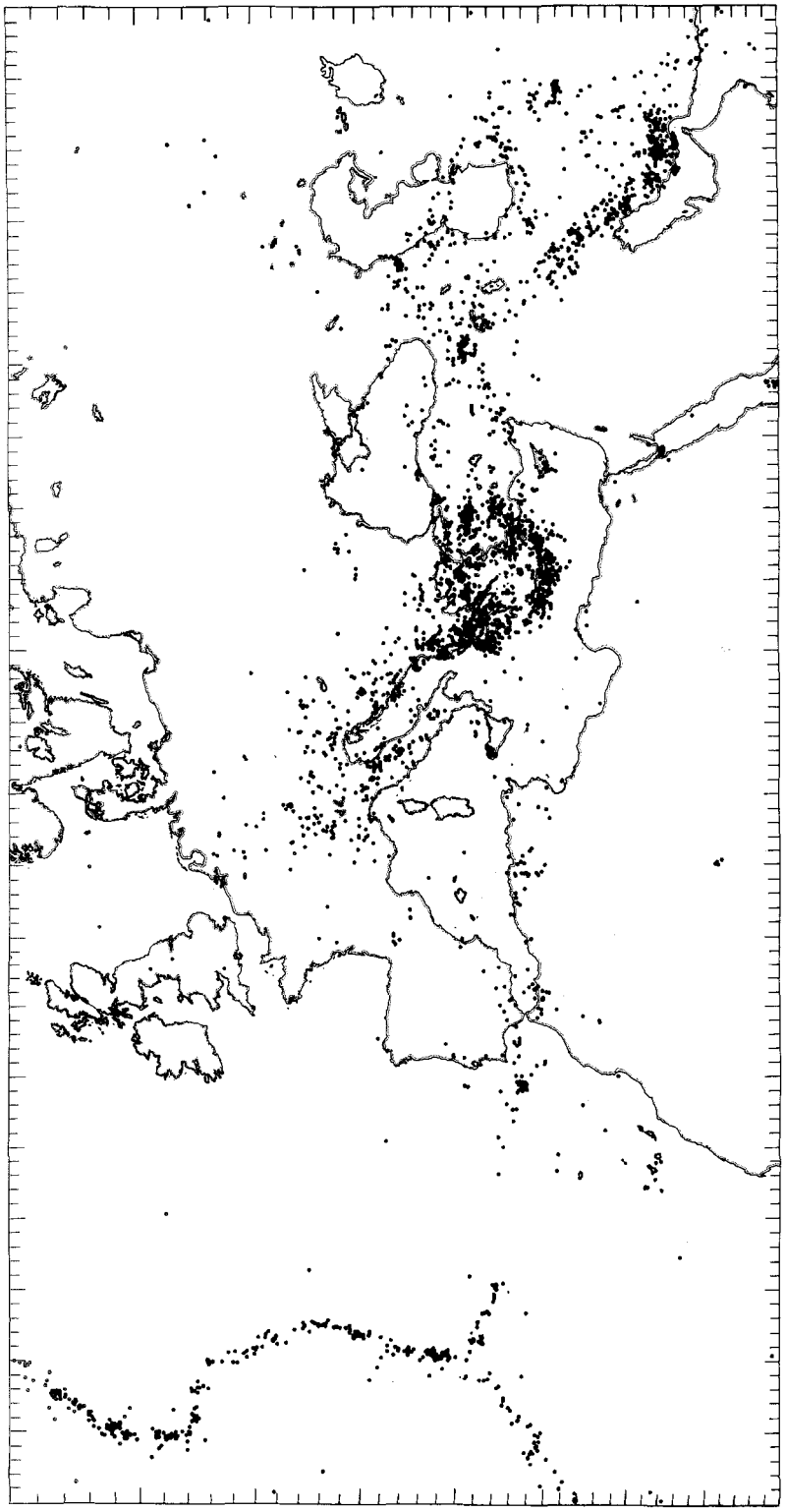

Figure 1. Positions of earthquakes between 1961 and 1974 between the Atlantic and Pakistan. Notice the difference in distribution between the oceanic and the continental regions.

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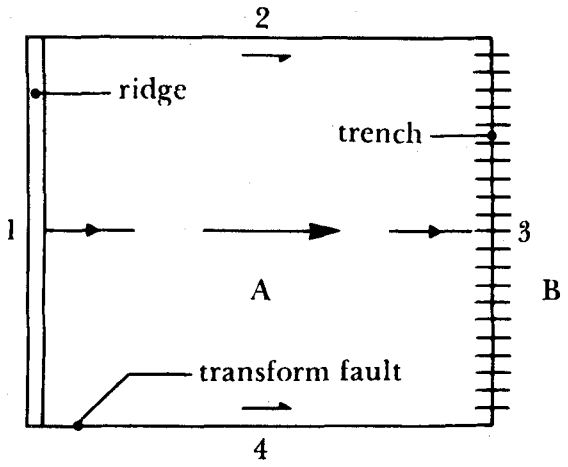

Figure 2. Plate A is moving to the right relative to plate B. It is being formed along the ridge (1), sliding on the transform faults (2 and 4), and being destroyed along the trench (3). The same symbols are used in the following figures.

aries are called *trenches* because they often form deep linear slots in the ocean floor. All well-developed ones are asymmetric and consume one or the other plate.

The motion in Figure 2 is a *translation* of A relative to B. This type of motion is particularly simple to understand, but it is not sufficiently general to describe all rigid motions of two plates because A could rotate as well as translate. The general case on a plane can always be described by a relative rotation about some point on the plane, and transform faults are then concentric circles centered on this point. We need the corresponding results for a sphere, which are that the general motion corresponds to rotation about an axis and that the lines of pure slip form small circles about this axis. This result is known as Euler's theorem and is in fact quite familiar in a different context: every day the Earth rotates about its axis and we move along small circles, or latitude lines, about this axis. The geophysical importance of this result was first recognized by McKenzie and Parker[4] and by Morgan,[5] although it was implicit in earlier work (see Section 6). The position of the pole of relative rotation and the rate contain all the information about the motion of two plates in three quantities, the latitude and longitude of the pole and the angular velocity. This simplicity makes the theory easy to test and useful to predict the tectonics along unsurveyed parts of the plate boundaries.

The principal difference between plate tectonics and the earlier concepts of sea floor spreading and continental drift lies in the explicit recognition of the importance of rigidity. Continental drift was not concerned with processes beneath the sea, and though the reconstructions of Wegener, Du Toit, and others depended on the continents forming rigid spherical caps, this was not recognized. Hess' concept of sea floor spreading was principally concerned with the processes on ridges and less with the rigid regions of plate interiors. Plate tectonics includes both concepts but without making any statements at all about

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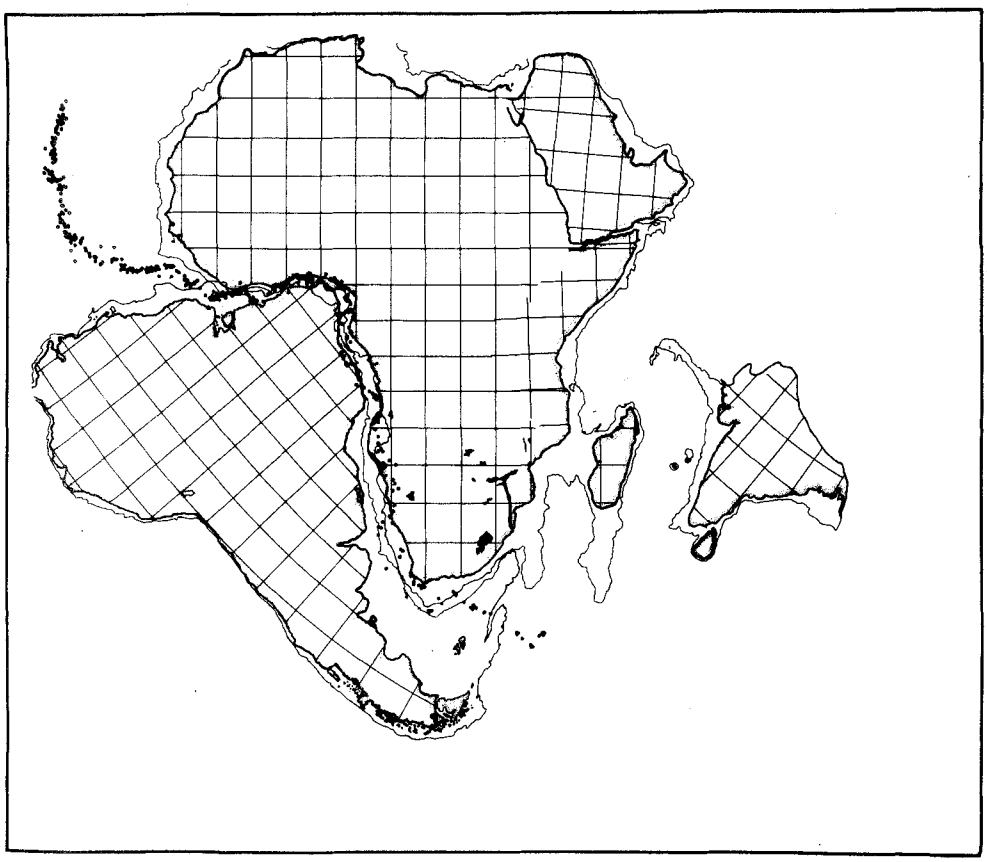

Figure 3. A reconstruction of the South Atlantic using a pole obtained by Bullard et al.[6] The present ridge axis marked by earthquake positions has been fixed and the two continents rotated through equal angles to show that the ridge follows the initial break.

the driving mechanism or the form of mantle convection. Neither can be directly observed, and neither are needed to produce a kinematic description of the observed surface motions. This separation of the kinematics from the dynamics was not made before plate tectonics was proposed, and it enormously simplified the whole subject.

The most straightforward demonstration that the plates move rigidly is that of Bullard, Everett, and Smith,[6] who used Euler's theorem to reconstruct the continents around the Atlantic before they were rifted apart. Figure 3 shows one of their reconstructions where the present ridge axis, marked by the locations of earthquakes, has been kept fixed and the two continents rotated through equal angles towards it. Because the ridge has added at equal rates to both plates it has remained mid-way between the two continents.

There are two main methods of finding the relative motion at plate boundaries. The first can be used only on a ridge and makes use of magnetic anomalies which cover most of the world's oceans. These are formed by a process first suggested by Vine and Matthews[7] and illustrated in Figure 4. Where the plates separate, hot mantle material upwells and melts to produce basaltic magma.

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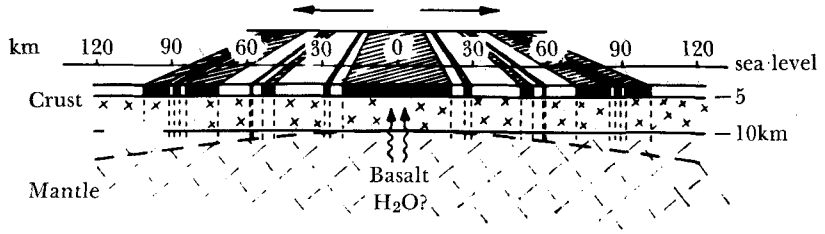
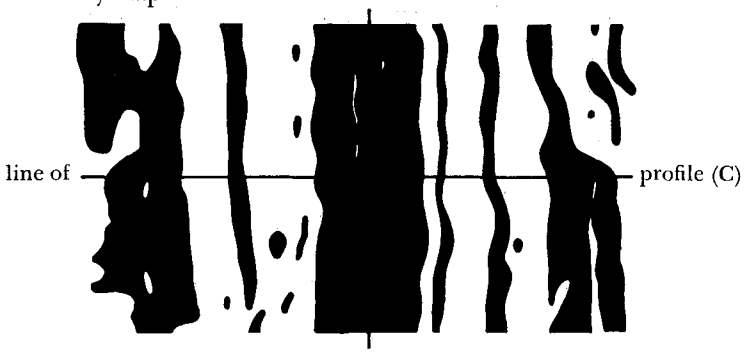
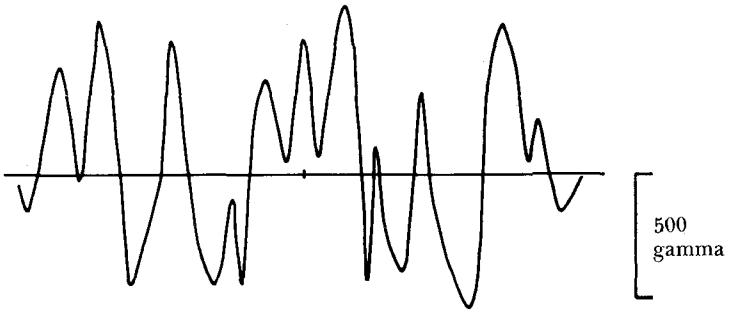
(A) Ridge model



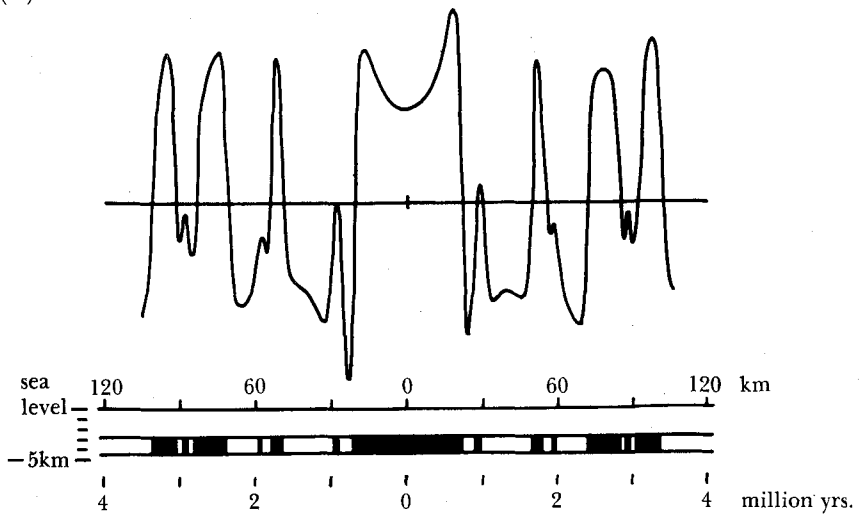
(B) Anomaly map



(C) Observed profile



(D) Simulation



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◀ *Figure 4.* (A) Model for the production of sea floor along the ridge axis. Shaded material is normally magnetized. (B) Part of the map of the magnetic anomalies shown in Figure 5. The black regions are areas of positive anomalies, the white are negative. (C) A total-field magnetic anomaly profile along the line shown in (B). (D) A profile computed from the reversal time scale assuming the model in (A). From F. J. Vine, "Magnetic Anomalies Associated with Mid-Ocean Ridges," in *The History of the Earth's Crust*, ed. Robert A. Phinney (Copyright © 1968 by Princeton University Press) Figure 1, p. 75. Reprinted by permission of Princeton University Press.

When this is erupted at the sea floor it cools rapidly and becomes magnetised in the present direction of the Earth's magnetic field. Fortunately this field reverses at intervals, and the north magnetic pole moves close to the south geographic pole, but fortunately not at periodic intervals. As the sea floor spreads it acts as a tape recorder and records the history of the field reversals as magnetic anomalies that can be observed at the sea surface by a magnetometer towed behind a ship. The reversal time scale can be established either by dating volcanic rocks on land whose sense of magnetisation has been measured, or by drilling holes through the sediment above the anomalies on the sea floor. This latter is the principal method by which rate of motion have been measured, and the corresponding directions are given by the strike of the transform faults, which are easily recognized by the offsets they produce of the ridge and anomaly pattern (see Fig. 5).

Neither transform faults which join ridges to trenches, nor trenches themselves, produce plates. The principal method used to determine the direction of relative motion is from the fault plane solutions of earthquakes. This method is less easy to explain than the generation of magnetic anomalies and is historically less important. It makes use of seismic records at about 100 stations on land to determine the direction, but not the rate of relative motion, between plates. It can be used only for earthquakes which occurred in this century and so is useless for making reconstructions. But since plate boundaries are defined by their seismicity it does, in principle, allow the direction of motion to be determined on all plate boundaries. Our knowledge of plate destruction comes almost entirely from the locations and mechanisms of earthquakes associated with trenches.

The magnetic anomalies off the western coast of the United States and Canada (Fig. 5) were the first to be discovered (Section 6), and were later shown to be part of a much larger area covered by similar anomalies (Fig. 6). Most of the advances in sea floor spreading and plate tectonics have come from attempts to interpret this pattern (see Cox[1]), and that this is now possible in considerable detail is one of the major successes of the new ideas. The failure to recognize the symmetry of the pattern about the ridge axes shown by arrows in Figure 5 caused a considerable delay in the general acceptance of Vine and Matthews' suggestion (Section 6). The other problem was more profound. The huge offsets on the faults such as the Mendocino and Murray do not continue onto the continent. Their failure to do so and their apparent independence of the San Andreas fault (the fault on land and parallel to the coast in California in Fig. 6) was very puzzling. Many people doubted that the displacements measured by the magnetic anomalies were true offsets. The explanation of these difficulties became clear when it was realized that another plate, often called the Farallon

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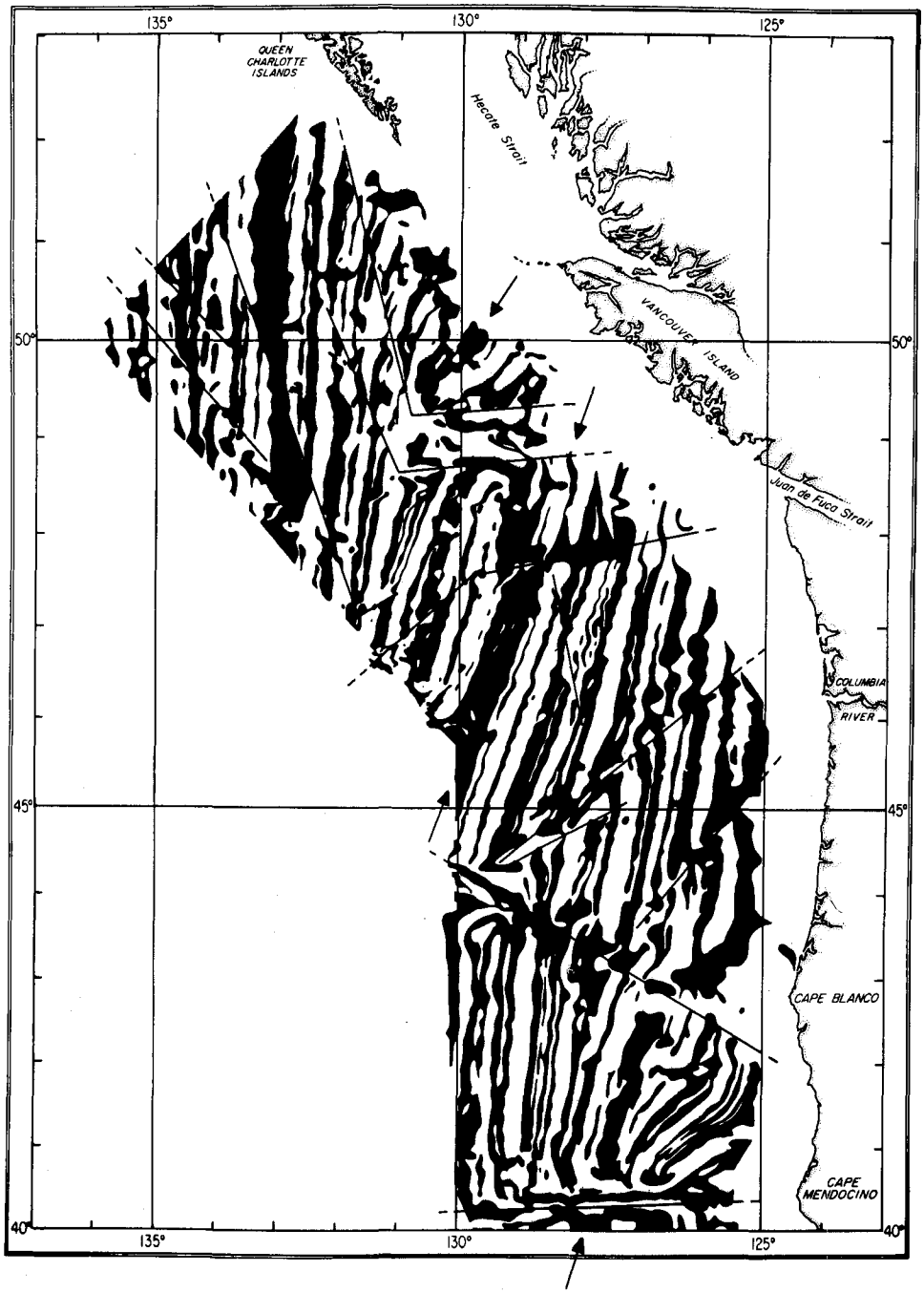

Figure 5. A map of the positive and negative magnetic anomalies off the West Coast of North America. Straight lines mark faults which offset the anomaly pattern and arrows the axes of three short ridge segments. (By permission from F. J. Vine, "Spreading of the Ocean Floor: New Evidence." *Science* 154:1405–1415, Fig. 1, 16 December, 1966. Copyright 1966 by the American Association for the Advancement of Science.)

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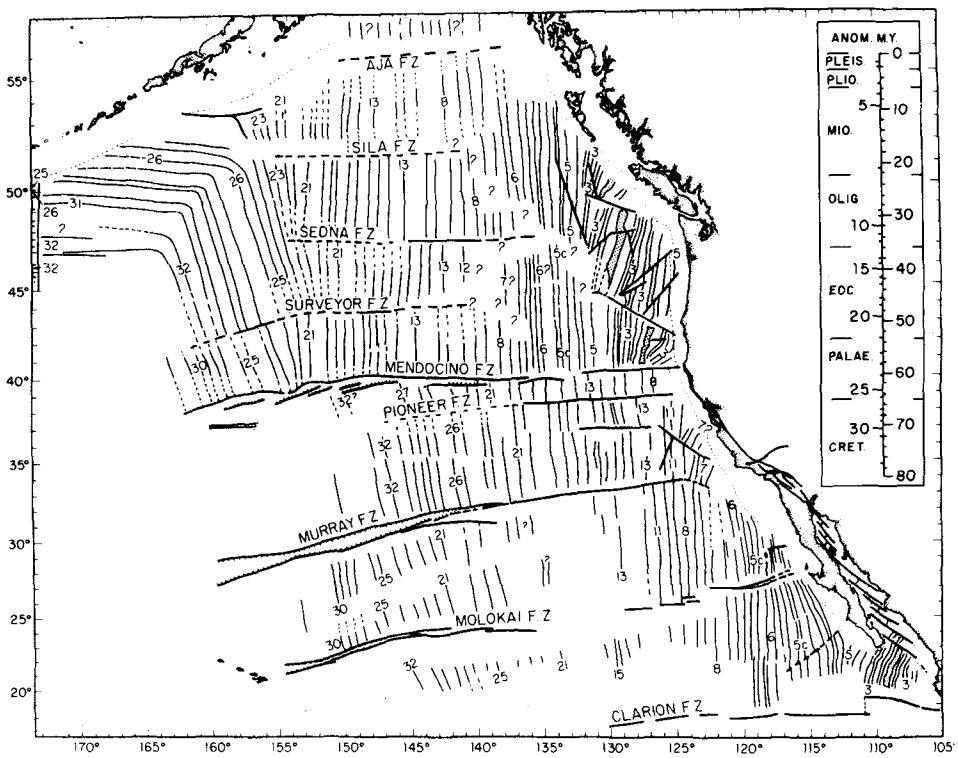

Figure 6. Magnetic anomalies in the Northeast Pacific, numbered in sequence of increasing age. The inset shows the approximate age of formation of each numbered anomaly. (By permission from T. Atwater, "Implications of Plate Tectonics for the Cenozoic Tectonic Evolution of Western North America." *Geological Society of America Bulletin* 81 (1970): 3513, Fig. 1.)

plate, existed between the American and the Pacific plates, of which only fragments now remain. Figure 7 shows how the magnetic anomalies and transform faults which offset them were formed by the motion between the Pacific and Farallon plates. The boundary of the American plate consisted of a trench which consumed almost all of the Farallon plate. As this happened the Pacific and American plates came into contact in a very complicated way, which can be worked out from plate tectonics alone (McKenzie and Morgan,[8] Atwater[9]). Even though this complicated evolution is undoubtedly too simple, because additional small plates are known to have been active, it did resolve all the major problems of the evolution of this region in terms of a three-plate model.

After the general acceptance of plate tectonics the major uncertainty which remained was whether it formed a useful description of continental motions. The problem is clearly shown by Figure 1. In the Atlantic the earthquakes are restricted to narrow belts but in the Mediterranean and the Middle East they are spread over a wide region which becomes even wider in Central Asia. There is at present no general agreement on whether plate tectonics can be applied to such deformation. Though I personally believe it can, a considerable number of plates are required (McKenzie[10]) and their shapes and number must constantly

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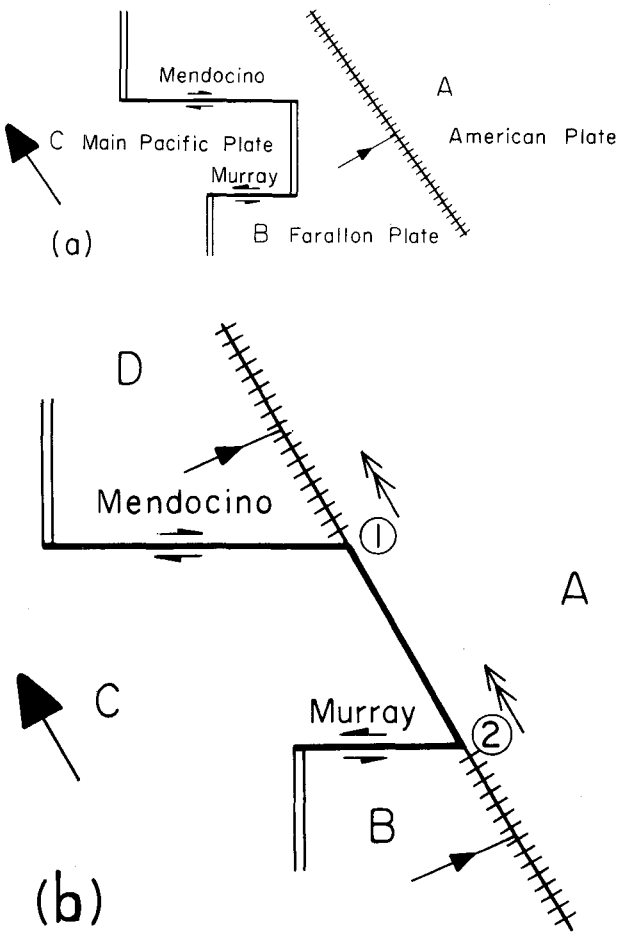

Figure 7. Two sketches to show the sequence of events which formed part of the anomaly patterns in Figures 5 and 6. The arrows on the Main Pacific Plate show its direction of motion relative to the American Plate. (a) Before anomaly 11 was formed, a continuous plate (the Farallon plate) existed between the American and Main Pacific plates. It moved westward relative to the Pacific plate and was consumed by a trench along the continental margin of North America. (b) When the ridge and trench met two points were produced where three plates met, called triple junctions, joined by a transform fault which later became the San Andreas Fault and which took up the motion between the Pacific and American plates. The motion of the triple junctions depends only on the relative motion of the plates and the geometry of the plate boundaries. (By permission from D. P. McKenzie and W. J. Morgan, "The Evolution of Triple Junctions." *Nature* 224 (1969): 125, parts of Figs. 5 and 6.)

change. Others, especially Molnar and Tapponnier,[11] prefer a model in which the deformation is continuous within a wide region, though they still recognize the importance of faults as surfaces of discontinuous motion. In practice the difference between these models is not very major and both are very different from anything which has been proposed for the deformation of the oceanic parts of plates. There is every reason to believe that continental deformation has al-

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ways been different from that of the oceans, and that this is the reason why tectonic geologists working on continental mountain belts did not appreciate the importance of rigidity. It was not a useful concept to describe deformation on the scale that they mapped, and it is not clear even now how useful it is on a much larger scale. There was no a priori reason to suppose that continents and oceans would behave so differently, and we still do not know why they do so.

3. *Tectonic Ideas around 1900*

Many of the observations used in the 1960s to support the new concepts were familiar to earth scientists active at the turn of the century, yet no attempt was made to construct and test any such theory. The most surprising and important concept which was already well known was that of isostatic compensation, which resulted from major advances in geodesy.

By the middle of the nineteenth century the science of map-making was very advanced. The principal instruments then used for this purpose were theodolites, which measure horizontal angles with great accuracy, and levels, which use the surface of a liquid to measure the difference in height between two points. The major problem of the method results from the fact that the gravity field of the Earth is not spherically symmetric, and therefore the line which is at right angles to the surface of a liquid does not pass through the centre of the Earth. Hence, if an observer tries to determine his latitude by (for instance) measuring the elevation of the pole star, he will in general get the wrong answer. Such errors are most obvious when the astronomical measurements are combined with geodetic surveys to find the radius of the Earth. Various such measurements in different parts of the world had given the radius with some accuracy, yet when the procedure was repeated in India by Everest (see Fisher[12]) the difference in latitude between two points was 5°23′42.294″ when determined using a theodolite, whereas astronomical measurements gave a value of 5°23′37.058″. This is a difference of only about 160 m in a distance of about 600 km, or a few parts in a thousand. Not only was the difference generally accepted but the Archdeacon of Calcutta, the Venerable John Henry Pratt,[13] showed by a laborious calculation that the mass of the Himalayas should deflect the vertical three times more than is observed at the more northerly point and that the difference should be about 16″. Airy,[14] who was the Astronomer Royal at the time, demonstrated that these observations were consistent with the existence of a low density root beneath the Himalayas which supported the mountains by protruding into a fluid material of density greater than that of the root. Pratt suggested an alternative model which also had a fluid layer at depth but whose upper surface was horizontal and the surface elevations resulted from thick low-density columns above this surface (we now know that both types of compensation occur). What is remarkable is that such a small discrepancy led to the general acceptance of a rather complicated model of the mechanical structure of the Earth, and that the chain of reasoning which did so should be so sophisticated. Though it was clear that the layer above the fluid had a considerable thickness and strength, there was no obvious way to determine either. A thickness of about 100 km was often used, but it was not well established. This layer

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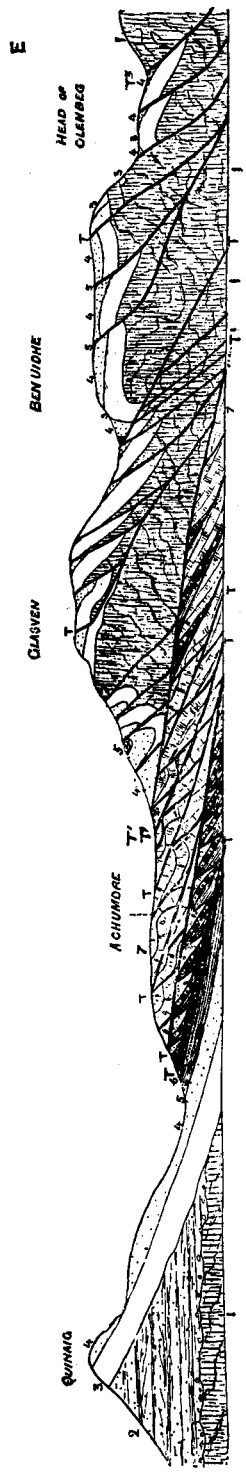
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is what we now call the *plate* or *lithosphere*, and the contrast between the willingness of most earth scientists at that time to accept this argument and the resistance displayed to the logical extension of these ideas based on much simpler arguments a hundred years later is extraordinary.

The significance of isostasy was clearly understood from the date of its discovery, but the same is not true of the other major discovery: the relationship between geological faults and earthquakes. In 1897 and 1906 two large earthquakes occurred on land, both of which were connected with movement along major geological faults exposed at the surface. Though the true importance of these observations was understood only when the full theory had become available (see Section 2), there is no obvious reason why the two reports on these shocks should not have directed attention to relationships between earthquake mechanisms and geological structures and to the global consequences of simple models such as that of Reid's (see Fig. 9).

The first of these reports is in retrospect the most impressive. In 1897 a huge earthquake, whose magnitude must have been about 8.7 on the Richter scale, shook the Shillong plateau in North East India. Oldham,[15] who was at that time Director of The Geological Survey of India, carried out a detailed investigation of the source region, which is very rugged and covered with jungle. Despite the problem of access Oldham succeeded in finding and tracing several surface breaks, one of which had a throw of 35 feet. He followed this large fault northwards until it vanished beneath the alluvium of the Brahmaputra Valley, where the only surface evidence of movement was a broad flexure. He suspected, probably correctly, that he had failed to find the surface trace of the main fault whose movement produced the shock. He believed that the fault was a thrust which emerged along the southern edge of the Shillong plateau, and compared this structure with that of the Assynt region of North West Scotland (Fig. 8). In this region Peach, Horne, et al.[16] had recently mapped a large thrust and its associated structures which has been deeply eroded since movement ceased about 400 million years ago. They had found that the rocks above the thrust plane were cut by many small thrusts with steeper dips (Fig. 8), and Oldham believed that the surface breaks he had mapped were produced by this type of smaller fault. He persuaded the Survey of India to retriangulate part of the region, but his enthusiasm does not appear to have been infectious. Only some of the stations were occupied and the results were inconclusive. His experience is typical: it is still very difficult to persuade surveyors to retriangulate after earthquakes, and often it is even harder to persuade them that the changes they find are real movements rather than observational errors. It is especially sad that Oldham was not successful, since there is little doubt that a careful survey would have demonstrated the correctness of his model. Unfortunately, even he was not convinced and later changed his ideas (see Richter[17]).

The 1906 Californian earthquake was in all respects simpler. It occurred on a structure called the San Andreas fault which had already been mapped in outline, and the shock was produced by a right-handed displacement of up to 15 ½ feet along the northernmost 190 miles of the fault. Most of the faulted region was open woodland of easy access, and for this reason the break was easily followed for much of its length. The results were collected in an impor-

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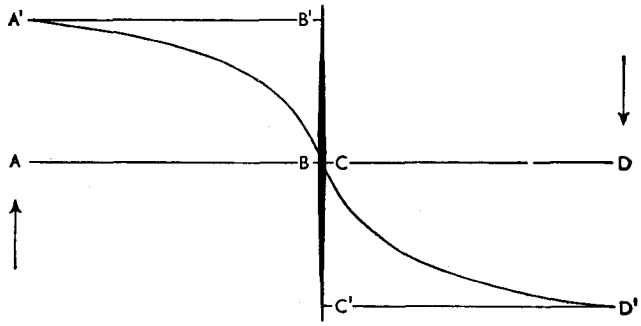

Figure 9. Diagram to show how continuous lateral movement leads to intermittent earthquakes (Reid's elastic rebound model). ABCD is initially a straight line across the fault marked by the heavy line. As the plates move A moves to A′, and D to D′ until the strain is sufficient to overcome the frictional resistance on the fault. When this happens B jumps to B′, C to C′, and an earthquake results. (By permission from A. Holmes, *The Principles of Physical Geology* (1st ed., London: Nelson, 1965; 2nd ed., New York: Ronald Press, Fig. 163.))

tant report which had an enormous influence on seismology. The authors, and particularly Reid,[18] recognized for the first time that the earthquake was produced by the release of elastic stresses which accumulated as that part of California southwest of the fault moved slowly northwestward (Figs. 7, 9). Triangulation confirmed that the motion was steady far from the fault, and that the sudden movements related to the earthquake did not extend more than a few tens of kilometres from the surface break. It must have been clear to the writers that the total movement on the fault had to be very large (later work demonstrated a displacement of at least 300 km) because the scale of the San Andreas Rift was very large compared with the features produced by the earthquake. No one seemed particularly troubled by what happened at the ends of the fault, though this problem is briefly discussed: the general view at the time was that faults could start and stop. It is surprising that the connection between the San Andreas fault and the Gulf of California was not taken more seriously. The other line of argument which could have led to the start of plate tectonics concerned the displacements at considerable distances from the fault, and on this question Reid is quite explicit. "We have heretofore only considered the region between . . . Mount Diablo and the Farallon Islands [the furthest extent of the triangulation], but we now extend our consideration to the whole region moved. It is evident that the displaced area must have some limit; the surveys only covered the region between [Mount Diablo and the Farallon Islands], and therefore throw no light on what occurred at great distances from the fault." It would be most interesting to know why Reid held this view. He offers no evidence for it, clearly believing it to be self-evident.

It is unfortunate that the relationship of the San Andreas fault to the other active structures around the Pacific is so complicated (see Section 2). An account by Holmes[19] (pp. 939–943) published nearly sixty years after the 1906 shock shows how little progress was made before the framework of plate tectonics became available. Even after the basic ideas had been accepted it was not imme-

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diately obvious how to explain the tectonics of California. This development can be followed in the collection of papers made by Cox[1] (Wilson[3]; Vine and Wilson[20]; Vine[21]; McKenzie and Morgan[8]; Atwater[9]); it led to the examination of the behaviour of points where three plates meet, and, only after this was understood, to a convincing description of the evolution of California. The only place yet known where the relationship between oceanic and continental structures is relatively simple is at the northern end of the Red Sea, where most of the extension is taken up on the Dead Sea transform fault. It is not surprising that the geologists working in this region came very close to discovering plate tectonics before 1967 (Quennell,[22] Freund[23]).

The problem in India was the lack of direct observations of movement on the major thrusts. No thrust faults were observed to break the surface in either the 1897 or in the more recent shocks, and indeed surface breaks appeared to be the exception rather than the rule. These observations caused several seismologists to doubt that all earthquakes were the result of slip on faults. Only when accurate focal mechanisms for shocks became available in the 1960s were their doubts resolved. These reasonable arguments why an early discovery was unlikely apply with equal force to isostatic compensation.

Perhaps the important difference was that most earth scientists accepted the existence of a fluid layer of molten lava at some depth within the earth in 1850 but had abandoned this model by 1900.

4. *Wegener and the Resulting Debate*

Wegener[24] was the first earth scientist to make a serious attempt to construct a theory resembling plate tectonics. He was not much concerned with the floor of the oceans, and concentrated on the evidence of large horizontal movements of the continents. For this reason his version is generally known as *continental drift*. His ideas generated an astonishing controversy: extraordinary public attacks were made on his scientific objectivity as well as on his ideas (see Hallam[2] and Van Waterschoot van der Gracht, et al.[25]). Wegener's main contribution was that he recognized that the geometry and geology of some continental margins was easily explained by enormous horizontal motions. The problem with his exposition was that he went too far. He included any observations, no matter how insecure, which favoured his argument, and glossed over the objections of his critics. Perhaps such an approach was necessary to get his ideas discussed, but it had unfortunate effects. As techniques improved several of the observations he used were shown to be in error: as more was discovered about the oceanic floor several of his suggestions became untenable. These failures inevitably cast doubt on the soundness of his conclusions. But I do not believe that these remarks can fully explain why Wegener's ideas failed to be accepted, or why his supporters failed to develop a comprehensive theory.

The group of people most likely to construct a rigorous theory were those (such as Jeffreys) working on the boundary between the earth sciences and mathematics. There is no doubt that the opposition of the few people in this group who had the necessary mathematical background seriously impeded the development of Wegener's ideas. Though a number of geologists rejected Jef-

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freys' arguments, they did not succeed in refuting them, and Jeffreys' complete opposition to large horizontal displacements still remains.

Most of the effort during the 1920s and 1930s was devoted to the theory of elastic wave propagation and its application to detemining the interior structure of the Earth. This work was outstandingly successful: Jeffreys and Bullen produced a spherically symmetric model for the velocity structure for the Earth's interior which has scarcely required any modification, and much was discovered about the geography of earthquakes. Not surprisingly this success dominated their view of the mechanical properties of the Earth's interior. The simplest and most important result was that shear waves propagate through the mantle to a depth of 2900 km. Since shear waves cannot pass through a liquid, no layer of molten rock can exist in the outer parts of the Earth, and the model which had been suggested by isostatic compensation needed modification. That compensation occurred was generally agreed: by this time the evidence for it consisted of a large number of gravity measurements which, unlike the geodetic observations which led to its discovery, were not very dependent on the accuracy of the triangulation and leveling lines linking the stations. Hence there was no reasonable doubt that flow occurred within a solid mantle.

Jeffreys[26] (p. 178) gives a good summary of what was known at the time, and clearly distinguishes between a fluid, which cannot propagate shear waves; a solid at high temperatures, which creeps slowly under any stress however small but which can propagate shear waves; and a solid at low temperatures, which deforms elastically and does not creep until the stress exceeds a finite value. Jeffreys believed that solids acquired finite strength within a few hundred degrees of their melting temperature. We now know that these ideas are substantially correct and understand the atomic processes which are involved. Jeffreys[26] used these ideas to discuss isostasy (pp. 161–202) though he made little effort to apply his mathematical development to estimate the thickness of the outer elastic shell of the Earth. He supposed it to be between 30 and 300 km, and modern estimates based on Jeffreys' model and analysis have shown that a thickness of about 30 km gives good agreement with gravity and topography. Clearly, Jeffreys was very close indeed to reconciling a wide variety of geophysical observations with large horizontal motions, yet his discussion of Wegener's ideas (pp. 319–325) shows that he himself believed otherwise. This appendix is of considerable interest: Jeffreys objects to a comparison between his relationship to Wegener's supporters and that between Kelvin and the nineteenth-century geologists. Many years earlier, Kelvin had estimated the age of the Earth to be about a fifth of that believed by geologists at the time, and only a hundredth of the age now accepted. Kelvin's error was the result of ignorance: radioactivity had not been discovered. Yet to a modern reader this comparison is very apt and the remarks from Huxley, which Jeffreys quotes, accurately reflect the views of Wegener's supporters then and now. Jeffreys then discusses his objections to Wegener's ideas, of which the most important was the lack of a mechanism. He was not convinced that much of Wegener's evidence was satisfactory though he does less than justice to the geometrical fit and was not really qualified to dismiss Du Toit's geological arguments. But the mechanism problem caused difficulties from Wegener's time onwards, and it is still by no means resolved. The

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great success of plate tectonics came about because it separated the problem of the kinematic motions from that of the driving mechanism, which is now widely believed to be a form of thermal convection. It is curious that Jeffreys not only made major contributions to the theory of thermal convection but also suggested[26] (pp. 292–293) that mantle convection contributed to the surface heat flow. Yet he still has not accepted either that large horizontal displacements exist, or that thermal convection can supply the driving force to maintain them. His opposition to these ideas had great influence until the 1960s, and played an important part in the development of the subject.

Though most geologists were opposed to Wegener's ideas, several of the leading ones supported them. The most influential of these were Du Toit, Holmes, and Argand. Du Toit worked in South Africa and had an unrivalled knowledge of its geology. He was much impressed by the similarity between the rocks and fossils of South America and South Africa, and in consequence became an enthusiastic supporter of Wegener's ideas. He produced an excellent reconstruction showing where the southern continents were about 200 million years ago which is still largely accepted. He collected his ideas in a book (Du Toit[27]) which had considerable influence, even though it is written in a curious style. He made little attempt at constructing a balanced scientific argument in favour of large horizontal motions. Instead he collected all the observations he could from every relevant discipline which could be explained by continental drift. He was not more successful than Wegener, though he did provide those few northern hemisphere geologists who were convinced of the truth of Wegener's ideas with some good arguments.

Holmes[28] was more sophisticated about his support of Wegener. In a carefully argued paper published in an obscure journal he showed that existing arguments against continental drift were not convincing and that mantle convection could maintain the continental motions. He supposed that the flow was driven by radioactive heat generated within the continents and by the ellipticity of the surfaces of constant density produced by the Earth's rotation. A detailed investigation of these forces and the resulting convection would have been very useful, and the necessary mathematical techniques were available. The only attempt to do so was made by Pekeris[29] at Jeffreys' suggestion, but Pekeris' analysis was not easy for geologists to understand and had little influence on the development of ideas. Holmes also did not influence the nature of the controversy to any great extent, and in the absence of any consensus the arguments between Wegener's supporters and the non-drifters became more extreme and personal.

Argand was one of a number of continental geologists to accept Wegener's ideas, and his views were certainly influenced by his intimate and extensive knowledge of alpine geology. No other hypothesis could, he believed, explain the enormous lateral movements observed in the Alps and the Himalayas, and he attempted to produce a description of the deformation using continental drift.[30] Unlike Du Toit and Holmes, Argand was concerned with the kinematic movements at the Earth's surface, and for this reason his synthesis of continental deformation closely resembles recent proposals (Molnar and Tapponnier[11]). The extent to which modern ideas of plate tectonics can be used to describe

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such deformation is still unclear, and Argand's account makes little use of the concept of rigidity. It would have been most surprising if Argand had seen its importance considering his area of interest.

What all Wegener's supporters did was to elaborate and illustrate his ideas: none of them recognized that his suggestions depended on the absence of deformation within the drifting continents. No geometric fits are otherwise possible. At first Wegener used the coastlines for fitting, and the fits were not very good; but later he recognized that the continental margins are more significant and their use greatly improved the fits. I suspect that the geologists did not appreciate the rigid behaviour because they were not expecting it. When two continents collide the local deformation is varied and complicated, and no one guessed that the behaviour of the oceanic lithosphere would be so profoundly different. It is much more surprising that Jeffreys should have overlooked the importance of rigidity than that Holmes and Du Toit did so, but perhaps it was because he was opposed to the whole concept. The discussion became rather emotional with personal attacks of one side by the other: it was quite common until recently to ask someone whether or not he "believed" in continental drift. It is surprising that none of the people who were later to make important contributions to continental drift such as Bullard, Hess, and Wilson made any serious attempt during this period to cut through the emotional debate surrounding the subject. Thirty years later they did so, but probably it was not considered wise for a young man to become involved in such a speculative enterprise during a world depression.

5. *Paleomagnetism*

It is now clear that the almost complete ignorance of marine geology which existed before the Second World War was the fundamental reason why so few people accepted that continental drift occurred. Before the war a few people had become interested in the sea floor, but they did not have the means to discover very much. The war established contact between the scientists and a few key people in the British and American navies, to the benefit of everybody. After the war the military importance of the oceans permitted the enormously expensive oceanographic work to continue. The immediate effect of the resulting work was an explosion of information: huge structures were discovered and mapped for the first time, some of which were quite unlike anything on land. It was, however, fifteen years before this new field had much influence on the ideas of continental drift, though when it did do so its impact was overwhelming.

An important development which could have produced the same effect was the investigation of the magnetization of rocks of different ages. This work became possible because Blackett[31] designed a very sensitive magnetometer which could measure the direction of magnetization of a rock sample. Measurements on recent volcanic rocks showed that their direction of magnetization was rarely more than a few degrees different from true north, and inclination agreed with that expected from a simple model of the Earth's field called the *axial dipole model* (Fig. 10). If the continents have drifted, then the directions of magnetization should differ, with the difference depending on the age of the rocks. The simple

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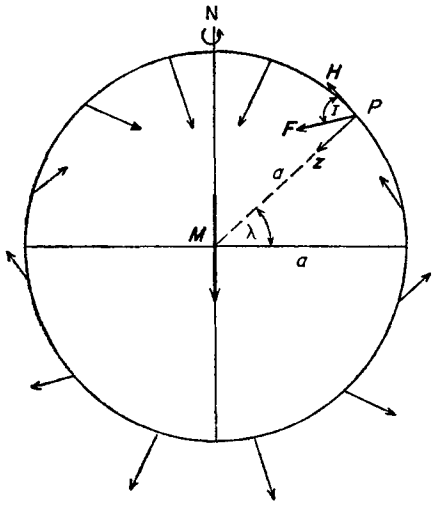

Figure 10. Model of the Earth's magnetic field used in paleomagnetic reconstruction. The arrows show the direction of the magnetizing field at different points. This model is called the axial dipole model. (By permission from M. W. McElhinny, *Paleomagnetism and Plate Tectonics* (Cambridge: Cambridge University Press, 1973, Fig. 16.))

model of the Earth's magnetic field in Figure 10 allows the direction and distance to the North Pole to be calculated from a single measurement. It is obviously sensible to make a number of measurements to reduce random errors, especially since the model in Figure 10 is known to be too simple. Nonetheless, paleomagnetic observations in principle can determine the relative motion between a plate and the Earth's poles, though they cannot determine the relative motion between two plates because the direction of magnetization is not affected by rotations about the Earth's rotational axis. It is, for instance, possible for the pole of relative motion between two plates to coincide with the North Pole, and for neither plate to move northward. Such motion cannot be detected by paleomagnetism.

This example shows that the relationship between the pole paths and plate motions is not straightforward, but it is obviously not possible for two parts of the same plate to have different pole paths. To test whether continental drift occurred a number of Blackett's students carried out a systematic investigation of European and North American rocks, and quickly found that the pole positions so obtained differed from the present pole position and that the difference increased with the age of the rocks. The first measurements were not accurate enough to show whether the two continents had moved with respect to each other as well as with respect to the pole, and it was possible to explain the results by polar wandering rather than by continental drift. Though more careful measurements showed that the two poles were somewhat separated, the case for continental drift was not very convincing until Irving (see Irving et al.[32]) determined the pole positions relative to Australia, which were quite different from the two northern continents (Fig. 11). Later work on other continents also required continental drift, and those involved were quickly convinced to the reality of large horizontal displacements. Furthermore, the pole positions ob-

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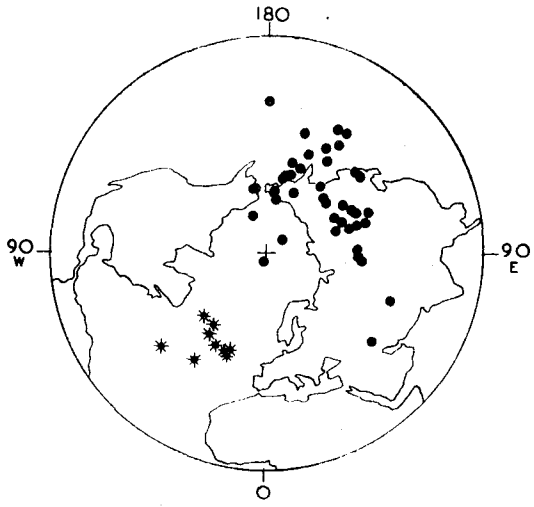

Figure 11. Mesozoic paleomagnetic pole positions from Australia (stars) and from Europe, North America, and Siberia (dots). (By permission from E. Irving, W. A. Robertson, and P. M. Stott, "The Significance of the Paleomagnetic Results from Mesozoic Rocks of Eastern Australia." *Journal of Geophysical Research* 68 (1963): 2313, Fig. 3. Copyrighted by the American Geophysical Union.)

tained from the major southern continents were compatible with Du Toit's and Wegener's reconstructions. The magnetic anomalies have since provided independent confirmation of these results, and paleomagnetism is now accepted as a major tool for disentangling horizontal displacements.

Why then did the early results fail to convince the majority of earth scientists? I believe that the principal reason was the complicated and specialist nature of the work, together with the number of untested assumptions which were required to interpret the magnetization directions. The investigations showed that some rock specimens gave very scattered directions, sometimes because the original magnetization had been altered by chemical processes long after deposition of the rocks, or sometimes because of the instability of the first magnetization. Gradually methods were developed to reduce these difficulties (for an account of modern methods in this field see McElhinny[33]), but a number of those who were not directly involved even doubted that a sample could be taken with a hammer without disturbing the magnetization.

A more fundamental difficulty, to which no one had a convincing answer, was the form of the Earth's magnetic field at earlier periods. Though the present field is relatively simple and compass needles point north with an error of only a few degrees at most places, there is still no generally accepted model for the Earth's dynamo which predicts this result. The only way round this difficulty was to assume that the geomagnetic field has always had the simple form of an axial dipole now observed. This assumption was very difficult to test using paleomagnetism alone, and those who doubted it could argue that the present field is in fact considerably more complicated than the one used to derive pole positions. Theoretical models are no help in this argument, and there are even now still doubts whether the Permian field was dominated by the axial dipole. Clearly anyone who chose to doubt the reality of the large displacements had a

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variety of arguments to support his view. In addition, there are two difficulties which are particularly fundamental and troublesome, and which still produce major variations among the reconstructions of different workers. It is clear from Figure 10 that the measurements give only the latitude and orientation of the rocks when they were magnetized, and not the longitude. For this reason paleomagnetic measurements can test a reconstruction, but they do not alone provide enough information to make one. The other difficulty is the scatter of the observations, which is rarely less than five degrees. The scatter is caused by a variety of effects including the non-axial components of the Earth's field. Though five degrees appears rather small it may represent an uncertainty in position of more than 500 km, which is not.

These were, I believe, the main scientific reasons why paleomagnetism failed to change most people's minds. The arguments were complex and uncertain; unlike in physics, in the earth sciences long and complicated chains of reasoning are rarely correct: there are too many effects which could be important, but which have been neglected. Furthermore, the observations were restricted to continental rocks and therefore did not resolve the two major problems which then troubled people: what drives the motions, and why is there so little evidence on the sea floor of continents ploughing their way across ocean basins?

But at least as important as these scientific questions were the doubts generated by the extreme certainty of those newly converted to continental drift. Runcorn[34] produced a number of elaborate convective models instead of trying to reconcile the objections of the marine geologists who were now becoming influential. The division between drifters and non-drifters which proved so unhelpful in the 1920s and 1930s reappeared.

6. *Uneven Progress Towards a General Theory*

Modern ideas of plate tectonics began around 1960 and their development was dominated by the marine geologists, who gradually realized that the geology of the ocean floor was quite unlike that of the continents. The typical metamorphic rocks of continental mountain belts are not found beneath the deep oceans, whose basement consists of igneous rocks. The sediments which overlie this basement are everywhere less than 200 million years old, or less than a twentieth of the age of the oldest continental rocks. The major structures which cross the ocean floor are also quite unlike anything known on the continents. Hess[35] (see Cox[1]) collected much of this information in a speculative paper which for the first time attempted to relate the structure of the ocean floor to the ideas of continental drift. In many respects this paper is less satisfactory than that of Holmes[28] thirty years earlier.

Hess did, however, make one suggestion which provided the essential clue to the development which followed. Continental drift requires ocean floor to be created behind the moving continent and destroyed ahead of it, though the way in which this was supposed to happen was never worked out in detail. Holmes believed that drift occurred by stretching over much of an ocean basin, whereas Hess suggested that new sea floor was created only along lines, the axes of the oceanic ridges. Elsewhere the continents and their surrounding oceans moved

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together. By this modification of Wegener's ideas Hess produced a model which was compatible with the observations of marine geology. Like Holmes, Hess argued that the motions were maintained by convection, and he suggested that the oceanic ridges are the surface expression of the rising part of the convection cell. The emphasis which Hess placed on the importance of crustal generation on ridge axes distinguished his ideas from those of Wegener, and for this reason his hypothesis is generally known as *sea* (or *ocean*) *floor spreading*.

The other major advance was an instrumental one. During the 1950s various groups developed magnetometers which could be towed behind a ship and which could make accurate and frequent measurements of the Earth's magnetic field. Raff and Mason[36] put such a magnetometer on board a U.S. Navy survey ship working off the West Coast of the United States, and to everyone's surprise showed that large magnetic anomalies were present which were very accurately linear and parallel to each other over hundreds of kilometers (see Fig. 5) except where they were displaced by faults. Most of these faults did not break up the pattern but offset it by large distances which were sometimes more than 1000 km (Vacquier et al.[37]). Nothing resembling these anomalies had ever been observed over the continents, and their origin was mysterious. Their importance was, however, generally recognized, and the huge offsets on the faults which cut the pattern was at once used as evidence of the existence of sea floor spreading. But the puzzling absence of any expression of these faults in the geologically well-mapped parts of the western United States took away much of the force of this argument.

The explanation in 1963 of these linear magnetic anomalies by sea floor spreading combined with reversals of the Earth's magnetic field (see Section 2) marks the start of the construction of the theory of plate tectonics. The idea apparently occurred to Morley and to Vine and Matthews[7] independently (see Cox[1]) and both used the same line of reasoning. Both noticed that the magnetic field over some seamounts could be explained only if the seamount was reversely magnetized. Raff and Mason[36] had noticed the same phenomenon (but not appreciated its importance) in their magnetic surveys off the western United States, but it was not obvious to others because the Navy had classified the bathymetric maps as confidential material. Vine and Matthews recognized that a seamount they surveyed in the Indian Ocean was reversely magnetized. Both combined this observation with Hess' concept of sea floor spreading to explain the anomalies. Neither could go much further because no detailed reversal time scale was available, though as soon as the necessary work was done on continental volcanic rocks their suggestions were strikingly confirmed. No one believed that a magnetic survey of a spreading ridge existed because the topography of Raff and Mason's survey was still unavailable.

The speed of these developments is very surprising, and contrasts greatly with the earlier arguments discussed in the last two sections. One of the main reasons for this was the improvement in international travel which occurred when the large jets went into service, allowing British and American scientists to meet frequently. Another reason was less obvious but perhaps more important. For the first time the observational marine geologists had a coherent theoretical model which depended on the existence of horizontal movements. The rapid discovery of isostasy in the nineteenth century took place under similarly

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favourable conditions: the existence of a continuous layer of lava at a depth of several tens of kilometers was part of the conventional wisdom of the earth sciences at the time. In both cases the theoretical models were not particularly accurate, but in both cases they allowed the key puzzling observations to be quickly and correctly understood.

Vine and Matthews' explanation of oceanic magnetic anomalies aroused little interest at first—even at Cambridge. It was not until 1965, when Wilson spent several months there working with Vine, that any attempt was made to use the Vine-Matthews hypothesis to unravel the history of the ocean basins. The main reason for this delay was that most of the magnetic profiles at Cambridge were from the North Atlantic, where the anomalies are not very distinctive. The other problem was the desire of all marine geologists to produce contour maps of the magnetic field, rather than working with profiles. These problems prevented Vine and Matthews from making much use of their idea. But in 1965 Vine and Wilson[20] recognized that a spreading ridge existed within the area surveyed by Raff and Mason[36] (Fig. 5) and that the Vine-Matthews hypothesis could be tested. They used the reversal time scale, which had meanwhile been established, to show that the calculated and observed profiles agreed reasonably well, though their correlation is not that which is now generally accepted.

Wilson[3] also at this time recognized the existence and importance of rigid plates and worked out a number of the consequences of their relative motion. This paper was the first to consider the process of destruction, as well as the creation of sea floor. The third of this series of papers to appear in 1965 was that of Bullard, Everett, and Smith[6] and was concerned with the problem of geometric fits of continental margins. Bullard has recognized that the most secure of Wegener's arguments was the matching of opposing margins, and he and his colleagues decided to produce the best fits they could. To do this they needed to be able to describe rigid motions on a spherical surface, and pointed out that one easy way to do so is to use Euler's theorem. They used this approach to produce some excellent fits, especially of South America and Africa (see Fig. 3), but they did not see the connection between this procedure and Wilson's ideas. This was not done until 1967, when Morgan[5] and McKenzie and Parker[4] put together Bullard et al.'s use of Euler's theorem and Wilson's concepts of rigidity to produce the theory of plate tectonics described in Section 2, which has not been appreciably modified since.

The period between 1965 and 1967 is more interesting than that which followed. By 1965 large files of data existed in various institutions which were later used to determine the relative motion between plates and the evolution of the ocean basis, yet earth scientists were very slow to do so, especially at the major American oceanographic laboratories. A particularly interesting paper in this connection is that of Heirtzler, Le Pichon, and Baron,[38] who carried out an aeromagnetic survey of part of the Mid-Atlantic Ridge. They found a symmetric pattern of magnetic anomalies, each of which formed a stripe parallel to the present ridge axis. This survey was used by Vine[21] later the same year to provide strong support for the Vine-Matthews hypothesis and sea floor spreading, yet in Heirtzler et al.'s paper this hypothesis is dismissed in two sentences. By the following year Heirtzler and Le Pichon had changed their views completely

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and went on to provide important confirmation of the concepts of sea floor spreading and plate tectonics (see Cox[1]), but their reaction to the earlier work was typical of U.S. workers who were not at Princeton. This view was almost universal until the evidence in favour of sea floor spreading became overwhelming during 1966, and is not easily explained. There is no doubt that, with the exception of Hess, most established marine geologists were opposed to speculation in general and continental drift in particular. But it is curious that this hostility had such a dominant influence on the work of younger scientists.

The general acceptance of and enthusiasm for sea floor spreading came in 1966, and the huge data banks which Lamont Geological Observatory in particular had collected over many years were quickly used to work out the evolution of the world's major oceans. Within a year sea floor spreading had become part of plate tectonics, and seismology had been used to investigate plate destruction. During this period progress was very rapid, sufficiently so to allow those involved to write logically sound papers. Unfortunately, this makes it much harder to follow the development of ideas. At the end of this period two major tectonic problems remained: how are the plate motions maintained, and why does the basic theory work so poorly in continental regions? But these questions are not part of the story, though they do explain why the early attempts of continental geologists who tried to relate the evolution of mountain belts to mantle convection met with little success.

7. *The Earth and Biological Sciences: Similarities and Differences*

The obvious comparison is between the impact of plate tectonics on the geological sciences and that of the discovery of the structure of DNA in the biological sciences. Both discoveries had an essential simplicity which made them easy to popularise, and both caused a massive influx of able young scientists into hitherto unfashionable subjects. But there are also important differences. Unlike the biologists and geneticists, who had no idea how genetic information was stored and transmitted at a molecular level, geologists had attempted to fit their observations into a framework of drifting continents. The background of those involved was also different. The major contributions in molecular biology were made by physicists and chemists using sophisticated techniques unfamiliar to biologists, whereas Hess, Matthews, Vine, and Wilson were basically geologists who acquired the necessary geophysical knowledge to attack marine geology. There was little difference between their approach and that of those with a training in physics such as Bullard, Morgan, Parker, and myself. The molecular biologists approached the subject in a new way, and, because of the enormous complexity of simple organisms, are still at a very early stage in understanding most biological systems. The relationship of the anatomy and functions of something as primitive as an insect still cannot be related to the amino acid sequence of its DNA.

The change produced in the earth sciences by the general acceptance of plate tectonics was not nearly so radical. It is clear that a number of geologists recognized that they did not understand mountain-building either in the past or at present. Plate tectonics provided a framework for describing present movements, and was quickly and enthusiastically accepted by geologists as a guide to much older structures. Though the scale of the processes involved was much

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larger, there was no fundamental difference between the events described here and the major discoveries of nineteenth-century geology, such as the recognition of past glaciations and volcanism. The concept of uniformitarianism—that the present is the guide to the past—has been the basis for all these advances. Unlike biology, much of geology and petrology was already soundly based on physics and chemistry. That it was possible to do the same for tectonics came as a surprise to everyone, and though Euler's theorem provided the essential starting point, the development necessary to describe the motions and interactions of several spherical caps did not exist and had to be worked out. The closest analogy to plate tectonics is the dislocation theory of crystalline materials, and even though the resemblance is not very strong it provided a guide to the type of kinematic theory which could be produced starting from the concept of rigidity. It is not surprising that those who were trained as physicists were prominent in the development of these ideas, but, unlike molecular biology, those with a geological training had no difficulty in understanding and using the new techniques.

Like molecular biology, plate tectonics suggested new interpretations of a mass of existing knowledge. The topography of ridges is clearly related to the contraction of plates as they cool and move away from spreading centres (Sclater and Francheteau[39] in Cox[1]). This subsidence in turn can account for the sediment composition observed in deep sea drill holes (Berger and von Rad[40]). The land connections between different continents allowed animals and plants to migrate and can account for unexpected features of their present distribution (Jardine and McKenzie[41]). But plate tectonics was less fundamental a revolution than the discoveries which began molecular biology, and the new ideas have therefore been assimilated and exploited faster in the geological sciences.

Within schools and universities the enthusiasm for the earth sciences which was such a striking feature of the nineteenth century has returned, and plate tectonics is being taught in high schools within ten years of its discovery. Here there is a close resemblance to the revival of biological sciences which happened about ten years earlier. Popular interest in the new ideas has become considerable, the BBC has made several films about plates, and the number of undergraduates in earth science courses has increased in number and quality. Not all of these changes have been produced by plate tectonics alone. Concern with environmental problems, the increasingly specialist nature of much of the modern work in physics and chemistry and its remoteness from everyday experience, and the desire to travel to remote parts of the world have all increased the attraction of the earth sciences. The effect has been to increase the ability and versatility of those working in the subject. Though new advances on a scale comparable to those described in Section 2 seem unlikely in the near future, many of the implications of the new ideas have not yet been worked out in any detail.

8. Where Next?

Shortly before he died Hess expressed the view that the exciting period was over in tectonics, and that all there was left to do was to mop up those areas where there were at present no data. This dispiriting view was premature; it certainly seems to be true for the oceanic part of plates but when plate bound-

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aries cross continental crust the resulting deformation is complicated and not restricted to one fault. Since land geologists study continental crust the distinction is important. The reason for this difference in style is not understood, though a number of speculations exist (Molnar and Tapponnier[11]). Until it is the tectonics of ancient and modern belts is not easy to relate to plate motions. Another obvious question to be settled is the nature of the driving mechanism. The success of the kinematic theory showed that surface deformation was dominated by plate rigidity. Though this is in one sense a great advantage, the strength of the plates obscures almost all expressions of mass motion below. It is now generally agreed that thermal convection is the energy source but the form the convection takes is not clear. Geophysical observations are difficult to make and little is known about the convective behaviour of fluids whose properties resemble those of the mantle (see McKenzie and Richter[42] for a recent review of our ignorance). But slow progress is being made, though the work is conceptually more difficult than is plate tectonics. Fortunately the driving mechanism is no longer of direct interest to those working on the kinematics.

The relationship between plate motions and the composition of lavas erupted at the surface was recognized from the beginning. Those associated with plate consumption on the whole contain more silica than those produced at ridge axes and were also believed to be more variable in their composition, though recent detailed sampling on the Mid-Atlantic ridge has produced a much greater variety of volcanic rocks than had been expected. Though everyone is agreed that these differences are consequences of plate creation and destruction, it has turned out to be surprisingly difficult to say very much more. As yet nothing is known about the shape of the reservoirs which contain the lava before it is erupted, which appears to be important. The other problem is that the relevant laboratory experiments are very difficult to carry out, and the results are rarely uncontroversial. Since all oceanic magnetic anomalies are younger than 200 million years old, the principal method of investigating older tectonic events is by using the composition of the volcanic rocks associated with them. But in the absence of an accepted model for their generation, any such extrapolation is obviously uncertain. This problem becomes especially important during the first half of the Earth's history. Some volcanic rocks formed at this time contain less silica than any now being generated. Because such rocks have been severely deformed by later events it is not clear what their relationship is to processes at plate boundaries, or even whether any parts of the Earth's surface moved rigidly. Though I would be surprised if plates had not existed at this period, the radioactive heat generation within the Earth was probably a factor of about ten greater and this difference should be reflected in the tectonics.

I had hoped that other planets in the solar system would display a variety of styles of plate tectonics, but it is now clear that my hopes were unfounded. With the possible exception of Venus the rocky planets of the inner solar system show little evidence of surface deformation except by meteorites and atmospheric processes. This is a major disappointment: the Earth's present behaviour would be easier to understand if we had a number of examples of different sizes and compositions. But Venus may yet help, especially because its surface temperature may be sufficiently high to prevent rigid motions, and therefore to display directly the convective motions which exist beneath the plates on the Earth.

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These problems can all be expected to yield to detailed investigations, but they are different in nature from plate tectonics. There is at present no proposal which divides earth scientists in the way that continental drift did earlier in this century. I think it is unlikely that history will repeat itself within the lifetime of the present generation of scientists: it is too obvious what obstructions such divisions caused. Every referee of a speculative paper and every editor of a journal knows the sad story of the rejection of Morley's paper, and is prepared to publish large amounts of nonsense to avoid repeating this mistake. The same is true of public attitudes to new ideas, which are not now rejected because of their apparent implausibility. Against this background future major advances are more likely to resemble those in the older more organized sciences like physics, where there has been a clearer division between what is and what is not understood at a particular time, and new theories are rapidly developed and tested once they have been proposed. They are then either adopted or discarded, but they do not now provoke long and emotional controversies. If I am correct, then Hess was definitely too pessimistic and neither his views nor mine on the directions in which future advances will take are of very much value, and the subject will have become more exciting, rather than the converse, now that the major controversy has been resolved.

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M. I. FINLEY

"Progress" in Historiography

I

In *Comment on écrit l'histoire*, a superlatively clever and aphoristic work that has produced something of an *éclat*, at least in France, Paul Veyne wrote that "Thucydides would have learned from reading what Burckhardt and Nilsson have written about his own civilization and his own religion. Had he himself tried to discuss those topics, his phrases would have been much poorer than ours."[1] The curious coupling of Burckhardt and Nilsson, analogous to coupling Max Weber (or even Arnold Toynbee) and Langer's *Encyclopedia of World History*, raises doubts. Nevertheless, beneath the flawed example there lies an important doctrine, the Crocean view, in Veyne's formulation, that "the *intelligence* of history has been enriched from the time of the Greeks to today, not because we know the principles or the ends of human events but because we have acquired a much richer casuistic of these events. That is the only progress of which historiography is capable"—progress in "simple description without method."[2] Elsewhere Veyne speaks of the "inventory," of the historian's "palette," and again of "conceptualization." "Herodotus and Thucydides had at their disposal all the facts necessary for founding social history or religious history, . . . which they did not found. It was the 'intellectual instruments' that were lacking."[3]

In one formulation or another, the Croce-Veyne doctrine would, I believe, receive widespread assent among historians today, with no doubt heavier stress on improvements in technique. The implications, however, are not fully drawn out, perhaps not even recognized. Historians, like members of other professions (if I may thus categorize historians), are reluctant to analyse themselves and their activity: they leave that to the philosophers, whose efforts they then dismiss as ignorant or irrelevant or both. In this essay I am compelled by my brief to look at the implications. "It is perfectly obvious," it was said to me, that "the whole field of social history"—by way of example—"has been vastly transformed in recent decades." How and why has that happened in the field of ancient history?[4]

But first, is that really the case? In several respects, undoubtedly:

1. The volume of data, the mere numbers of known facts, increase daily with the discovery and publication of hitherto unknown inscriptions, papyri, coins, and occasionally literary texts, and with the still continuing acceleration in archaeological exploration.

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2. Techniques are improving all the time, by the application of modern science in archaeology, by the use of the computer in lexicography, and so on. Today we can date documents and texts with a precision neither Thucydides nor Burckhardt could have managed, and we can graphically expose Thucydides' choice of words, phrases, and syntax as he himself could not have dreamed of.

Only in the most naive sense could the "whole field" of social (or political or religious) history be said to have been "transformed" by either improved techniques or a greater volume of data, or by both together. Thucydides would have learned thousands of facts about "his own religion" from Martin Nilsson's books, but it is to be doubted that he would have significantly altered his views about the place of religion in "history" as a result, and it is strenuously to be denied that his "phrases" would have been "much poorer" than Nilsson's on the subject. What could be "poorer" than the following characteristic generalizations by Nilsson: "the fate of religion is determined by the masses"; "it was only natural" that women "should apply to divinities of their own sex"; Polybius's famous dictum (6.56) that "superstition" is the "foundation" of Roman greatness "is philosophy and must be passed over in an exposition of *popular religion*"?[5]

Thucydides' smaller repertoire of facts cannot justify the argument that he lacked the "intellectual instruments." We must seek that lack, and the notion of "transformation," in two other, complementary developments in the study and writing of history.

3. Every historian suffers inevitably from ignorance of what will happen after him. Every historian, even the most mediocre, therefore has greater "historical experience" than his predecessors, however outstanding. That is a truism, but it is important. The point, of course, is not the banal one that new institutions arise in the course of "historical experience"; hence no fifth-century B.C. Greek could have conceived the Weberian doctrine of bureaucracy. The point is rather that subsequent experience makes possible, and stimulates, a reappraisal of older institutions within their own time and their own context. Had Thucydides known the history of Roman and later religions, he would no doubt have retained his contempt for oracles and their purveyors but he might well have treated the role of religious institutions differently, and at greater length (religious institutions, not gods, I hasten to say). Burckhardt, in other words, might have influenced his assessment of "his own religion" as Nilsson would not. Hindsight is an essential tool of the historian, not a vulgar joke at his expense.

4. The corollary of accumulating historical experience is a change, or at least a possible change, in stresses and explanatory models. Burckhardt's most brilliant "discovery" was the central place of the *agon* in Greek life (an untranslatable word, normally rendered by the pale "athletic competition" or by "struggle," neither of which captures the overtones as well as its English descendant, "agony"). I write "discovery" in quotation marks because all thinking Greeks knew it from the absence of the *agon* in other cultures with which they were in direct contact: witness Herodotus on Egypt (2.91) or Lucian's *Anacharsis*. But no Greek gave it the stress, the centrality, it possesses in Burckhardt's *Griechische Kulturgeschichte*.

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And so there is a deep divide between Livy's *History of Rome* and Mommsen's *History of Rome*, between the *Annals* and *Histories* of Tacitus and Rostovtzeff's *Social and Economic History of the Roman Empire*.[6] No one doubts that, but our concern is with modern historiography. More than a generation separates Burckhardt from Nilsson, yet, if I am right, the "transformation" in this instance has been retrogressive. Nor is that all. Hardly any professional ancient historians today read the *Griechische Kulturgeschichte* (or if they do, they fail to acknowledge it), as if Wilamowitz's notorious dismissal of the work—"für die Wissenschaft nicht existiert"[7]— has become a binding law. Nilsson, on the contrary, is hailed as the greatest twentieth-century authority on ancient Greek religion: the footnotes in most books on the subject attest his eminence in abundance. That there is a positive correlation here between intellectual calibre and professional repute cannot be maintained today by even the most severe critic of Burckhardt. Something much more fundamental is involved—about the nature and condition of historiography, not merely about two individuals.

II

"Historical experience" cannot be codified. Acton thought otherwise, when he launched the Cambridge histories, but he was quickly proved wrong: the more recent UNESCO attempt, its multivolume *History of Mankind*, was widely, and correctly, derided as both an anachronism and a *reductio ad absurdum*. There are no Kuhnian paradigms, no established doctrines about *wie es eigentlich gewesen*, not even about the *es*, the historian's subject matter. How, then, is it possible to envisage, let alone agree about, "transformation" or "progress" in this field? Who speaks for the ancient historians, today or in any previous generation?

A recent evaluation of Jacob Burckhardt concluded with these words: "Burckhardt's very personal conception of a cultural history of antiquity, grounded in universal history, can no longer be overlooked in ancient history. His widening of the circle of historical sources, his employment of art as one element in *Geistesgeschichte*, . . . his penchant for historical abstraction and for the comparative analysis of historical forces, the multiplicity of his independently won, new discoveries, which have proved to be valid and fruitful, assure him a place among the great individuals in the historical discipline, despite the fact that his historiography could not found a school. . . ."[8] Although I have much sympathy with that assessment, I am compelled to call it a vain hope, not a statement of the current realities in the field (apart from the seven final words of the quotation).[9] With a small number of notable exceptions, as I have already said, Burckhardt's *Griechische Kulturgeschichte* does not exist for professional ancient historians. Grave criticisms of some of his most essential conceptions, of the existence of a "Greek *Geist*" in particular, can properly be levelled, but that they do not constitute a sufficient explanation of the neglect is evident from the obvious, non-controversial aspect of his contribution. No one can deny that the visual arts are "one element in *Geistesgeschichte*," but few give that truism more than lip service when they write about the Greeks and Romans.

A more complex instance of the "it-is-no-longer-possible" fallacy is provided by Mommsen, the indisputable *Meister* of Roman history, unrivalled in his own day and today.[10] He himself considered it one of his greatest achievements to

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have helped to close the traditional breach between juristic and historical study. "The era, in which the historian wished to know nothing about *Rechtswissenschaft* and the jurist merely dragged in some historical research, in which the philologist thought it a waste of time to open the *Digest* and the Roman lawyer knew nothing of ancient literature other than the *Corpus Iuris*, . . . belongs to the past, and it is perhaps my merit, and above all my good fortune, to have been able to contribute to this liberation."[11] Yet in 1963 Momigliano opened an address to a congress of legal history with these words: "We are here, I feel, to celebrate an historical event of importance, the end of history of law as an autonomous branch of historical research . . . it is now clear to almost everyone that we can no longer maintain a distinction between historians' history and jurists' history."[12] He was not wrong, I believe, to accuse Mommsen (by implication) of misplaced confidence. As with Burckhardt on art, the "liberation" has been no more than superficial—and I fear that Momigliano's "historical event of some importance" is also a mirage.

The first volume of Mommsen's *Staatsrecht* appeared in 1871, three years after Burckhardt embarked on the course of lectures which emerged as the *Griechische Kulturgeschichte*. It required both courage and perception for Momigliano to see an essential link, not a mere coincidence, in the two dates—a return to "the systematic form characteristic of the antiquarian," but with the major qualification that they both sought "significance" and "essential principles," the one of the Greek spirit, the other of the organic Roman state.[13] It required courage not so much because Mommsen shared Wilamowitz's negative judgment of the *Griechische Kulturgeschichte* but because their respective places in the history of historiography about antiquity were (and are) at virtually opposite poles. Momigliano himself surely feels more kinship to, and agreement with, Mommsen; yet the perception points in the right direction. Reviewing Mommsen's first volume in the *Literarisches Centralblatt* for 1872, Ludwig Lange protested against Mommsen's "*Dogmatik des römischen Staatsrechts*," denied Mommsen's claim that he had opened the path to "rational progress" in the field, and predicted that Mommsen's authority would encourage the younger generation of students to impose his *Dogmatik* on the sources.[14] Other voices were raised from time to time, and eventually books were written that broke from the *Dogmatik* and the method, that rejected the organic Roman state and the "*Metaphysik des Staatsrechtes*"[15]—Ronald Syme's *The Roman Revolution*, for example, published in 1939, or, in particular and on a vast scale, De Martino's *Storia della costituzione romana* (about which I shall have more to say), which began to appear in 1951. Yet, as late as 1976, in a book on the relations between Republican Rome and the Italian communities, with the Weberian title, *Herrschaft und Verwaltung im republikanischen Italien*, a younger historian, Hartmut Galsterer, chose (and obviously felt it necessary) to announce that his "point of departure is the already quite old discontent with Mommsen's *Systematik*."[16] The discontent is no doubt old, but it is still ineffectual as well: although I have no statistics, I shall risk the statement that the *auctoritas* of Mommsen is called on more often in current writing on Roman history and law than that of any other historian. Does Mommsen then speak for the profession today? If so, where is the "transformation"?

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III

The more correct phrasing of the first of two questions I have just posed is: Which Mommsen speaks for the profession today? In 1902 Mommsen received the Nobel Prize for Literature (an honour shared only by Winston Churchill among historians), and the main ground for the award was his *Römische Geschichte*, an early work (1854–1856) which has met with an equivocal response, or worse, in the profession ever since its publication. Mommsen deliberately "modernized" Roman history and was open in his "subjective value judgments," thus converting his history "also into political pedagogy." The response was a "strong resonance in the broad reading-public" and a "peevish rejection by many colleagues."[17] In our own day, the *Staatsrecht* has been reprinted by the Wissenschaftliche Buchgesellschaft, the *Geschichte* by a leading German paperback publisher.[18]

The initial, divided response, we must underscore, was in the mid-nineteenth century. Even after due allowance for the proportion of the peevish rejection that is attributable to the substance of Mommsen's value judgments rather than to his presentation, the residue raises serious doubts about the fashionable view that, in Veyne's words, "The present development of historical studies in all western countries is characterized by the effort to move from *histoire événementielle* to a *histoire dite structurelle*."[19] What are Mommsen's *Staatsrecht* and Burckhardt's *Kulturgeschichte* if not a *histoire dite structurelle*? Neither exemplifies Lévi-Strauss's structuralism or the *Annales* kind of structuralism, to be sure, but that is a distinction of a different order from the one that, we are told on all sides, marks the "newest trend" in historiography. Nor is the *Staatsrecht* or the rest of Mommsen's prodigious oeuvre in legal history, epigraphy, textual edition, and numismatics less "historians' history," less "difficult for the layman to understand," than the tables and statistics, the polemics and the computerized charts of the new quantitative history; they are merely different in certain respects. I do not accept that such differences account for the widening "gap between the products of historical scholarship and the educated public." If the gap is widening in fact, if a "fundamental change in the relation between the historian and the educated general public has taken place"[20]—I leave the truth of that proposition open—the explanation must be sought elsewhere, in the mass production of *Wissenschaft* and the accompanying changes in the psychology of historians, in their image of themselves and their place in society. Even Wilamowitz knew that superlative skill in abstruse technical scholarship was not incompatible with a capacity for intelligible communication to a wider audience. "Philology," he once wrote, "is for philologists; whatever is immortal in Hellenism is for every man who wishes to come, to see and to grasp," and each week he gave a two-hour public lecture in Berlin, an event in the life of the city.[21]

I also do not accept the view, tempting though it may appear, that the professionals' desire for *histoire non-événementielle* runs counter to the "purposes of a public that still craves narrative accounts."[22] Kitto's *The Greeks* is by far the best-selling work on the subject since the Second World War, and my little book, *The Ancient Greeks*, has also had a much wider circulation than any narrative account, both in ten or a dozen languages. Previously there were G.

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Lowes Dickinson's *The Greek View of Life* and Alfred Zimmern's *The Greek Commonwealth*. However, my present concern is with the professionals, not with the "public," and I submit that they, too, continue to "crave narrative accounts." I do not underestimate the welcome novelty of the *Annales* school, or of such a history as A. Andrewes' *The Greeks*, in which "Outlines of Political History" constitute a single chapter, followed by seven with such titles as "Tribes and Kinship Groups," "Traders, Craftsmen and Slaves," "Social Values and Social Divisions." Nor do I underestimate the difficulty of locating this newer historical writing in the mainstream of the study and teaching of ancient history.

As one test, I have chosen three works in three languages, not at random (to claim that would be pretense) but also not, I trust, from a desire to score debating-points. One is the *Grundriss der römischen Geschichte* (to A.D. 284) by Hermann Bengtson, published in 1967 in the prestigious series, which he now edits, the *Handbuch der Altertumswissenschaft*, for nearly a century the acknowledged codification of the state of knowledge in all branches of classical studies (implicit in the title, "Handbook"). The second, Albino Garzetti's *From Tiberius to the Antonines. A History of the Roman Empire A.D. 14–192*, is a much longer, more leisurely account, which first appeared in 1960 and which was deemed worthy of an English translation in 1974 by two distinguished publishers, Methuen and Harper & Row, both active in the academic market as well as in sales to the general reader. The third book, also substantial, is the most recent, Raphael Sealey's *A History of the Greek City-States ca. 700–338 B.C.*, brought out late in 1976 by the University of California Press.

The first thing to be said about all three is that they are not only doggedly *événementielle* but also doggedly old-fashioned: "events" are what they have always been—kings and emperors, soldiers and consuls, wars, treaties, bits of legislation, conspiracies, and assassinations. Sealey does not even bother with lip service to new trends. The year 399 B.C. is for him the year in which Dercylidas succeeded Thibron in command of the survivors of the Ten Thousand (from Xenophon's *Anabasis*), not of the trial of Socrates (who is not once mentioned in the book). The other two make fleeting concessions. Bengtson has a 4-page chapter called "Roman Society in the Middle of the Second Century B.C.," most of which is about the impact of Stoicism on Roman political thought—immediately followed by a chapter of the same length on the Spanish wars, 158–133 B.C. Garzetti devotes more space to the non-question, why was Nerva chosen emperor in A.D. 96 and why did he accept?, than to any social, economic, or cultural topic.

The two it-is-no-longer-possible propositions I discussed earlier—art is part of *Geistesgeschichte* and legal history is history—are totally falsified in these books. There are three references to the Parthenon in Sealey's index: one to a picture, one to its evidentiary value in a scholarly dispute about the date of a papyrus fragment, the third to a list of Athenian temples and their dates designed to show that the Athenians did not "look forward to war with Sparta." There are two entries under "art" in Garzetti's index, one a page and a half about the "creative activity of Hadrian," which scarcely goes beyond a listing of monuments; the other, "characteristics under A. Pius," proves to be no more than the following sentence: "The spiritual mood was basically one of tiredness, reflect-

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ed in art in the tendency to follow a mannered classicism—the beginning of the 'late antique.' " A 17-page chapter in Bengtson on the culture of the Principate runs though Seneca, religion (including ruler cult), education, *collegia*, gladiators and Greek games, theatres ("Pompeii even had two"), trade, marriage, urbanization, and lots more. The same emptiness marks all three books on the law and jurisprudence; an enumeration would be boringly pointless.

I know of no easy way to determine whether it is these histories, or Andrewes', that truly exemplify the mainstream of ancient history today, or, indeed, whether there is a mainstream. One control is to examine the specialist periodicals. Since the war, the big journal devoted solely to ancient history is *Historia*, published in Germany but open to international scholarship. The latest complete volume (1975) has 50 articles (not counting two surveys of Soviet publications), of which two are about prehistory and only two others can by any stretch of the imagination be located outside the narrowest, most conventional *histoire événementielle*. The titles of the remaining 46 roll on with relentless similarity, from the first in the table of contents (arranged alphabetically by author), "The Aspirations of Q. Arrius," through "Ciceros Kritik an Sulla in der Rede für Roscius aus Ameria," "A Time-Table for the 'Bellum Neronis,' " "Un Milliaire de Trébonian Galle trouvé à Castiliscar," and so on, to the last one, "Zur Ankunft Julians in Sirmium 361 n. Chr. auf seinen Zug gegen Constantius II." That I have not been trapped into generalizing from the choices made by a single board of editors is shown by a similar breakdown of the two most important of the British classical journals which have a large historical component: the *Journal of Hellenic Studies* for 1975 (the most recent available) and the *Journal of Roman Studies* for 1976. There the number of relevant articles is smaller, but the concentration of subject-matter is identical.

For the presumed new trends one must normally turn to the scholarly analogues of the "little magazine," such as the recently founded *Quaderni di Storia* (or for classical literature, the now established *Arion*). Or one turns to articles on ancient history in non-specialist journals, the *Economic History Review*, *Past & Present*, the *Annales*, *Comparative Studies in Society and History*, or even the *American Historical Review* or the *Historische Zeitschrift*. That sharp division of labour raises further problems. Is ancient history perhaps outside the mainstream of current historical writing? I shall not attempt to consider that in this essay, but I cannot refrain from noting the pitiful quantity of the articles about antiquity that appear in such journals as I have just mentioned, despite the editorial welcome awaiting them.

It may be argued that I have been unfair to concentrate on "histories of" and on the specialist journals and to ignore the many monographs, on ancient social or economic history, on ancient religion and culture. Two things can be said in reply. The first is that the existence of such monographs is not at all a new phenomenon. The first edition of Augustus Boeckh's magisterial *Die Staatshaushaltung der Athener* was published in 1817, of Henri Wallon's two-volume *Histoire de l'esclavage dans l'antiquité* in 1847, of Ludwig Friedlaender's three-volume *Darstellungen aus der Sittengeschichte Roms* between 1862 and 1871; Julius Beloch's unsurpassed *Die Bevölkerung der griechisch-römischen Welt* appeared in 1886, Paul Guiraud's *La Propriété foncière en Grèce* in 1893, Rostovtzeff's *Studien zur Geschichte des römischen Kolonates* in 1910. The list can be extended to

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considerable length, and cannot be matched, for synthetic works on such a scale, in the most recent half-century. Cornford's Principle of Unripe Time has taken charge: the search for, and publication of, new evidence receive absolute priority, even of the most insignificant documents.[23] Bengtson summed up the psychology in his short introductory chapter on his predecessors, when he said that Mommsen's real contribution to *Forschung* (research, or better, Research) was not his *Römische Geschichte* but his *Staatsrecht* and the *Corpus Inscriptionum Latinarum*.[24]

The second point is that, though no historian is obligated to write a history of Greece or a history of Rome, on any scale, those who do so necessarily expose their whole conceptual system, their view of history, and their values. These are the works addressed not to fellow-*Forscher* but to a broader reading public (inside or outside the academy) and, above all, to the next generation. There can be no gap here "between the products of historical scholarship and the educated public": the arcane technicalities can be left out, as they regularly are. The residue is what such spokesmen of historical scholarship hold to be significant, interesting, relevant. Here, if anywhere, "transformation" should make itself evident. Until it is demonstrated that the authors of these histories are a peculiar breed, a skewed sample of the profession, I feel fully justified in concentrating on them in the present discussion.

IV

For the modern reader, Bengtson explains, Mommsen's *Römische Geschichte* is puzzling, "because it introduces him simultaneously to *two* pasts, to the time of the Romans and to the era of the political struggles of the nineteenth century." Better two pasts than none, it is tempting to retort, but I shall confine myself to the implicit illusion that Bengtson's own brand of positivism, his determination to be factual and nothing but factual, is less deeply rooted in his own twentieth century than was Mommsen's overt "political pedagogy" in the nineteenth. The mere allocation of space, the ineradicable series of imperial reigns as the containers of post-Republican Roman history, the loud silences on great chunks of human behaviour are proof enough that "objectivity," freedom from "subjective values," is pure illusion. The barest bones of any historical narrative, the events selected and arranged in a temporal sequence, imply a value judgment (or judgments) that flow from the historian's understanding of "relationships to longer enduring factors which are not themselves links in the sequential chain of events which constitute the 'story.' "[25]

The study and writing of history, in short, is a form of ideology. That blunt statement will shock, and I must make my meaning explicit. I speak of ideology neither in one of its doctrinal senses (Marx's, for example, or Mannheim's) nor in the familiar sense of the legitimation and support of a system or institution ("the ideology of Augustus")—all to be found in some historical writing—but in the broadest, "neutral" sense, roughly as defined in the *Shorter Oxford English Dictionary*: "a system of ideas concerning phenomena, esp. those of social life; the manner of thinking characteristic of a class or an individual." Hence I do not equate the ideology of professional historians, at least not of most of them, with the crude, politically motivated distortions, falsifications, and suppressions that marked what passed for Italian history throughout the

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Mussolini era or that converted Trotsky into a non-person in Soviet historiography.

The difficulty is then to determine when a "manner of thinking" becomes a distortion (or the more familiar "bias"), an omission a suppression. In other words, where is the line that separates legitimate, and indeed inescapable, intellectual behaviour—no man is free from a "manner of thinking" and no historian can record all known facts—from reprehensible (professional and moral) behaviour?

Let us take ancient slavery as a test, and let us assume that the role of slavery in ancient society is an open question. What do the three histories I have been examining reveal?

The word "slave" does not appear in the index of Sealey's *History of the Greek City-States*, and not through oversight. For instance, following the Peace of Nicias of 421 B.C., Athens and Sparta concluded an alliance with the following provision: "If there should be an uprising of slaves (i.e., helots), the Athenians shall come to the aid of the Spartans with all the strength at their command" (Thucydides 5.23.3). Sealey ignores the clause. The original charter of the League of the Hellenes, founded by Philip II of Macedon after he had gained control of Greece, specified that "there shall be no killing or banishment contrary to the laws of each city, no confiscation of property, no redistribution of land, no cancellation of debts, no freeing of slaves for purposes of revolution" (Pseudo-Demosthenes 17.15). Sealey's paraphrase reads (p. 490): "The constitutions in force in the member states when they joined the League were guaranteed; federal action was to check any acts of subversion or aggression against member states." Earlier, in a page (352) describing the new men who rose to political leadership in Athens following the death of Pericles, a page that draws heavily on W. R. Connor's book, *The New Politicians of Fifth-Century Athens*[26] (which, he writes, "provides an important inquiry into the nature of the political career in Athens and the change in style brought about by Cleon"), Sealey ignores Connor's long section, "The Social Standing of Cleon and His Successors," where it is argued (as George Grote had done more than a century before) that a shift had occurred among the men of wealth and leisure active in politics, from the rentier in the strict (landed) sense to the rentier living off the wealth produced by slaves in manufacture. As for Cleon's rival Nicias, Sealey fails to evaluate (again unlike Grote), or even mention, the strong ancient tradition that his great wealth came from 1000 slaves whom he let out to mining operators.

Bengtson and Garzetti, as I have already indicated, are more concessive in appearance. But then, not even Sealey would have been able to ignore the three great slave revolts in the period 140–70 B.C., two in Sicily and the uprising led by Spartacus in Italy, unmatched in history except by the Haitian struggle under Toussaint L'Ouverture. (Sealey manages to write a whole page, 256, on the helot revolt against Sparta in the 460s B.C., in which the helots are barely mentioned.) So Bengtson offers the reader three pages, not much more than on Pompey's brief war against the Aegean pirates based in Cilicia. These pirates were perhaps the main suppliers of slaves for Italy and Sicily, both then and earlier, when the revolts occurred, yet Bengtson sees no need to mention that, let alone consider the implications or the consequences of Pompey's victory on

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the slave supply. Nor does he notice a prominent event early in the reign of Nero, which Tacitus thought worthy of several pages (*Annals* 14.42–45): the prefect of the city, Pedanius Secundus, was murdered by one of his domestic slaves, and the attempt to enforce the law, according to which all the slaves in the household—a total of 400—should be put to death in consequence, led to serious street riots and an important juristic debate in the Senate, until Nero intervened personally and the mass execution proceeded. Garzetti allows that a bland sentence, but neither he nor Bengtson includes in their lengthy accounts of the "activities" of the second-century emperors the upsurge of imperial enactments aimed at facilitating the pursuit of fugitive slaves in the provinces.[27]

Most of the items in this catalogue of omissions are, or pertain to, "events," not to controversial "sociological interpretations." Their omission of course implies an interpretation, the validity of which I do not discuss. My concern is with the method, the preference for silence instead of presentation and evaluation of data that other historians hold to be significant (in books which freely engage in scholarly argument over the well-worn chronological puzzles, for instance).

It may again be objected that my sample is untypical, and I therefore turn to Joseph Vogt's *The Decline of Rome*, first published in 1965 in German.[28] Two considerations single this book out as a particularly significant test: it is not a narrow *histoire événementielle* (though it is no structural history either), and, more important, Vogt has for more than two decades been the chief inspirer and patron of a massive research effort into ancient slavery. By the time *The Decline of Rome* appeared, he had written many articles on the subject himself and had commissioned a substantial number of monographs by other German historians. The introductory chapter briefly summarizing "theories about the end of the ancient world" that "will be kept steadily in view in this survey of the period A.D. 200–500" includes a jejune page on the "Marxist" view of the role of slavery. "Steadily in view" then turns out to be an unfulfilled promise. Everything Vogt has to say about slavery after the introduction, if printed *seriatim*, would not occupy half a dozen pages. Putting aside such incidental facts as Constantine's legalization of manumission in a church (merely listed in a catalogue of that emperor's legislation in favour of the Church), we are left with the following remarks which I set out, omitting only minor elaborations or details and largely retaining Vogt's own words.

1. Already in the second century A.D., "the spark of private initiative was soon quenched. Moreover, little was done to improve techniques in the established branches of industry . . . science and technology had fallen into a decline and few fresh discoveries were made. It is insufficient to point to slavery as the reason for this stagnation. We may grant that a manufacturer employing slave labour had less incentive to look for improved and faster methods of work than a modern capitalist. . . . But slaves were disappearing from the industrial and rural scene, and manufacturers would certainly have reaped some advantage from new mechanical processes with which to eke out their labour force" (p. 25).

2. In the Roman west in the fifth century, there was a great "concentration of landed property" in the hands of the senatorial class. "Finding labour to work these large estates presented enormous difficulties. Wage-labourers . . . were

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now a rarity; the main burden fell on slaves and coloni. We find that on estates in Italy and Spain slaves predominate, although they are less numerous in Africa." As the once main source, prisoners of war, "had long since started to dry up, . . . the number of slaves reared on the estates considerably increased, in comparison with what it had been formerly. Their numbers were augmented by tragic victims of the class-state," infants who were exposed and children sold during a famine. "There were thus many slaves still available for agricultural work or employment as masons, carpenters, spinners and weavers." Ten lines follow to exemplify the "generalization" that "the condition of the unfree was approximating more and more closely to that of the free proletariat" (pp. 195–196).

3. "Slaves were another much sought-after form of merchandise. It is not known whether slaves were still needed in large number for employment in workshops, since in fact the general role of slaves in the industry of the Roman empire is still in need of clarification; but there was still a big demand for household slaves, . . . and (even) poor people liked to have at least one menial." Six lines follow on the trade and transportation of "all this material and human merchandise" (p. 201).

4. "The influence of the Christian ethic on legislation, which we have already noted in the time of Constantine, became still more pronounced. Some alleviation was brought to the slave's lot, in that manumission was encouraged and churches opened as places of asylum. . . . The law now extended the protection it afforded to the married state and the family to include the ménages of the unfree" (pp. 202–203).

Not a word could not have been written, and indeed they all were in older generations, before any of the modern publications on ancient slavery. The suspicion therefore arises that the much praised research programme sponsored by Vogt (and I do not underestimate its value) is essentially a marginal activity, which ancient historians are free to ignore the moment they turn back from specialized articles and monographs, or from social and economic history, to their "real" work, the "history" of Greece and Rome. How is it possible for a historian to adopt the same technique of interpretation by omission we have already discussed, when, in another role, he wrote articles with such titles as "Slavery and the Ideal of Man in Classical Greece" (1951), "Human Relationships in Ancient Slavery" (his Inaugural Address as Rector of the University of Tübingen, 1958), or "Slaves and the Liberal Arts in Ancient Rome" (1967)?[29] Who opened his *Rektoratsrede* with the words, "Slavery was an essential element in the social structure of antiquity"? Fewer than half a dozen pages out of 300 do not suggest "an essential element." Were Vogt unique in this matter, one could escape my suspicion by taking refuge in individual psychology. But he is unique only in the sense that he has devoted so much of his working life to the study of slavery. Something more widespread and comprehensive is involved than one man's psychology (or views).

We are witnessing an upsurge in the study of slavery that probably cannot be matched in any other historical field, for its volume, for the quality and sophistication of the analysis, for the innovatory techniques of inquiry—and for the fever-tone of the polemic, whether the subject is slavery in the southern United States or slavery in the Caribbean and Brazil, or the effects of the slave

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trade on Africa. Deep contemporary concerns underlie the tone and the intensity. David Brion Davis recently referred to the "coercion of the times," as a result of which "each new interpretation of slavery has professed to be more antiracist than the ones it replaces."[30] But the parallel leap in the study of ancient slavery cannot fall into quite the same class. In no direct sense are there contemporary social or political ills that can be blamed on the ancient Greeks and Romans. Nor is there the slightest shame or embarrassment among Greeks, Italians, or "Gauls" today because of their slaveowning "ancestors" of two thousand years ago. Partly, the interest in ancient slavery and the polemical tone of the discussions are a mere overspill from the renewed interest in more modern slavery, and partly they follow from the trend towards more study of social and economic institutions. However, I think both these considerations are secondary to two others.

A quick survey of the historiography of ancient slavery reveals three distinct phases. For most of the nineteenth century no one questioned the importance of slavery in antiquity, though not everyone shared Wallon's total commitment to the abolitionist cause. In the introduction to the second edition, published in 1879, Wallon wrote: "This book was produced at a time when the question of slavery was still to be resolved everywhere except in England. I proposed to bring into the debate the teaching of antiquity." But by 1879 there was no longer much interest in the slave past among Europeans in general and among European academics in particular. Furthermore, there were two more powerful pressures among students of classical antiquity which pointed in the opposite direction from Wallon. So successful were these pressures that ancient slavery virtually dropped from sight to the end of the Second World War, with remarkably few exceptions among professional historians.

The first of these pressures I can best identify as "the glory that was Greece and the grandeur that was Rome." Now that the long domination of the classics in liberal education was being seriously challenged, it became unseemly to sour the cream with slavery. For those who could not bring themselves to pretend that slavery did not exist, the solution, first discovered in 1898 by Eduard Meyer, was to insist that the Greeks and Romans had an unusually "mild" form of slavery, not to be compared with modern Negro slavery; that there were relatively small numbers of slaves, who were well treated and given ample opportunity to obtain their freedom.[31] The powerful combination of Meyer's *auctoritas* and the will to believe was made irresistible by the second pressure, the development of Marxism. Given the Marxist evolutionary scheme, most sharply formulated in Engels' *Origin of the Family*—primitive communism, slavery, feudalism, capitalism, socialism—it was effectively axiomatic among Marxists that Graeco-Roman society rested on a slave base. Axioms are axioms: Marxist historians (unlike Marx himself) showed little interest in pursuing that proposition in any detail, while non-Marxists and anti-Marxists were equally content to rest on Eduard Meyer, whose lecture, they thought, amounted to a disproof of Marxism as well.

The few exceptions were largely scholars pursuing technical pieces of research, on slave names, for example, as scholars do all the time without worrying over the larger implications. A few men, in both camps, went further, but their approach marks them as forerunners of the third phase that began not

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long after the Second World War. Much the most voluminous and most important work in this phase, on the non-Marxist side, has been the programme I have already mentioned, organized by Joseph Vogt, which included, in addition to a continuing stream of original publications, several translations into German of substantial volumes originally published in the Soviet Union. Inevitably such a programme produces work of great unevenness, in quality and in outlook, ranging from the pedestrian to the first-rate, from "neutral" scholarship to overt Cold War polemics.

Nevertheless, I believe I am justified to single out two functions. One is an important modification of pre-war anti-Marxism: the Meyer denial has been replaced by detailed "objective" study, breaking the Marxist monopoly of the study of ancient slavery, making certain concessions but coming down firmly for the view that Marxism is still shown to be a false doctrine on this central question. The other function is to rescue the German tradition of "classical humanism." If "the humanism of classical studies is to survive in our world," Vogt writes, "it must portray ancient society as it really was without concealing or extenuating its negative aspects." The Greek *polis* was a "miraculous creation." Slavery was "part of the sacrifice which had to be paid for this achievement"; it was "essential to the existence both of this basic will to live and of the devotion to spiritual considerations."[32] Those are what matter in history, what history is about, whereas "the Marxist approach to history . . . can only approach the subject in terms of material production and the creation of a class structure."[33] Yet that is precisely the one area in which Vogt himself mentions slavery in his *Decline of Rome*, apart from a pathetic attempt to find effective, "humanitarian" Christian influence on the institution in the later Empire.[34] Hegel, he had written a decade earlier, "was surely right to say that in the servility of the serf the master also loses his humanity."[35] But not, apparently, in the later Roman Empire. Research into ancient slavery during the period 1970–1975, a review-article concluded, has rarely extended the inquiry "beyond the manifestations (*Erscheinungsformen*) of slavery" in order to examine "its influence on the general social, economic, political and cultural development in antiquity."[36] That is further indication that the new attention to slavery has been marginal to the "real" business of the ancient historian.

Professionalism for its own sake, the cult of Research, is an ideological stance, too. If no ingredient, no "theory," is added other than a revulsion to Marxism, no serious concern with the broad canvas of the past is advanced, nor is fundamental social change illuminated. Everything becomes mere contingency. The tone might be called a modified Panglossism: all change is for the worse in the best of all possible worlds, except change in the historian's own technique—progress in "simple description without method."

Because I believe that the spectre of Marx overshadows current trends and thinking in ancient history, as in other fields of history, I shall briefly consider one more case-study, Francesco De Martino's *Storia della costituzione romana*. The first volume was published in 1951, the final one in 1967, and the whole was subsequently revised for a new edition completed in 1975. It is an iron law that any six-volume work of scholarship (more precisely in this instance, six volumes in seven) is by definition a major historical work. But De Martino is not just any scholar: while holding the chair of Roman Law in Naples and carrying

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on a continuous programme of research, he was simultaneously active in the Italian Socialist Party, eventually succeeding Pietro Nenni in the secretaryship (until the Party's disappointing results in the election of 1976). His history of the Roman constitution is intended to be a Marxist alternative to Mommsen's *Staatsrecht*. Its reception merits a much fuller analysis than the summary account which follows.[37]

We need waste no time on the merits of the work. Invidious comparisons with Mommsen or detailed statements of De Martino's debt to Mommsen are beside the point: no one can possibly write about the Roman constitution without leaning heavily on Mommsen, and no one will again be in a position to rival that very great pioneer. Nor are the nature and orientation of De Martino's Marxism at issue in the present context. It is sufficient that all discussions of the work treat it as Marxist, and that even reviewers unsympathetic to the point of view have acknowledged its value, resting as it does on massive annotation (comprising both the ancient sources and the modern scholarship): "The virtues of the book, which are overwhelming, are these. It is sensible, very well arranged, very clear and readable."[38]

Nevertheless, only in France and Italy have the professionals devoted the attention to the work that one might expect, and it is difficult to avoid the conclusion that this is a reflection of the current division, along national lines, in the status of Marxism among intellectuals in general and academics in particular. In those two countries alone has there been an appreciation of the importance of this alternative to Mommsen's *Systematik* and *Dogmatik*, as well among Romanists who reject De Martino's Marxism and defend the "juridical delineation of general principles."[39] It was one of the latter who said expressly that, just as Mommsen wrote as "a man of 1848," it is neither "illegitimate" nor "remarkable" nor "risible" that De Martino should write as an *uomo politico*. "Science," he concluded, "progresses more through dissent and discussion than by mere adherence to other people's ideas. We must be grateful to De Martino for having compelled us to rethink a series of fundamental problems."[40]

Neither "overwhelming virtues" nor "gratitude" nor my "iron law" have ruffled the calm waters of scholarship in Germany or the English-speaking countries. There the commonest reaction has been either a thundering silence, for which I can offer no parallel, or an equally astounding blindness: "almost entirely derivative," "inoffensive, if wordy, orthodoxy"[41]; "its point of view is traditional"[42]; "except for a slight flurry of Marxism . . . there is little to distinguish this . . . from any other . . . book on the constitution of the Roman Empire."[43] Even if that were the case, which it is not, I should be at a loss to explain why the work is not so much as listed in the bibliographies of book after book published during the past quarter century. *Il catalogo è longo:* Leporello could not record it and I shan't try. One example must suffice. In the jubilee volume of the *Journal of Roman Studies* (1960), a survey of "Fifty Years of Republican History," which opens and closes by announcing that "our purpose here is to show the trends of scholarship throughout the period," manages not to discuss De Martino in a single sentence (though the work is included without comment in a list of 18 titles in one footnote).

That same issue of the *Journal* carries a review of two of the volumes together, the theme of which is that De Martino's "strong Marxist leanings," his "preconceived views of history," "preclude an altogether impartial assessment

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of the evidence." The line between "impartial assessment" and agreement with the reviewer's own interpretation is not easy to draw. For example, "it is difficult to escape the conclusion that the author's chief reason for denying the unitary character of the pre-Etruscan monarchical regime is simply that he is precluded by his philosophy from *admitting* the existence of *a feudal state within which there was no discernible class conflict*"[44] (my italics). There is no notion of "progress through dissent and discussion" here, no willingness to "re-think a series of fundamental problems." Indeed, the reviewer comes dangerously close to the accusation of deliberate suppression and falsification.[45]

The reception as a whole bears witness to the truth of the proposition that "fellow practitioners reserve an uncomfortable spot in the limbo of the trade for those who fall from the grace of ambiguity to the sin of positive assertion."[46] No one charges Nilsson with failure to assess the evidence impartially, with suppression, with "refusal to admit," when the word "slave" does not appear in the index of his 1500-page work on Greek religion in the *Handbuch der Altertumswissenschaft*, despite the fact that an inquiry into the slave in ancient religion, sponsored by Vogt, turned up enough material for a four-volume monograph.[47] Yet Nilsson is no freer from "preconceived views." Where does the difference lie?

V

One undeniable, almost banal, conclusion is that there are sharp differences within the profession in fundamental conceptions, about the nature of historiography and about the ancient world itself, the subject-matter of that historiography, perhaps more so in the past half-century than ever before. There are, indeed, more differences and viewpoints than I have mentioned: I am not writing a *bibliographie raisonnée*. There are also undeniable "transformations," new interests and new approaches that are genuinely innovatory, not merely gimcrack novelties. But may we speak of progress, and, if so, in what sense?

If it is correct that historiography is an ideological activity, then much of the answer is self-evident. Ideologies change, and so the writing of history undergoes constant "transformation." It always has: in antiquity itself, one need only remember the sequence Herodotus-Thucydides-Polybius-Livy-Tacitus-Dio Cassius. But progress is a value-judgment, which in this instance rests on one's judgment of the historian's ideology. If, for example, one believes it to be a misjudgment of social behaviour to seek the mainsprings in the personalities and decisions of political and military elites, then the alternative analyses and explanations of some contemporary historians represent progress. Improvements in technique seem to me a minor side-issue in this context. So, to a large extent, is the distinction between *histoire événementielle* and some other kind, whether the latter is called structural or serial or quantitative or econometric. It is either another distinction in technique or a concentration on certain phenomena because they lend themselves to new, sophisticated methods. All the possible statistics about age of marriage, size of family, rate of illegitimacy will not add up to a history of the family; nor does the debate over *Time on the Cross*[48] (or any side in it) tell us enough that we need to know about the road to the American Civil War.

Arguments about the economics of cotton culture in relation to the Civil War preceded "econometric history," even in good old narrative political

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history-writing. Political history or social history, political history or economic history, narrative or institutional history—these seem to me essentially false choices, distractions from the genuine questions. Nothing I have said in this essay is intended to denigrate political history: its legitimacy is beyond reasonable challenge, even from those historians who, on whatever grounds, prefer to specialize in other fields. My concern has been with the orientation of current work in ancient political history, and Cleon provides a neat symbol. Every book on almost any aspect of classical Greece must deal with him. Does the historian also notice that Cleon represented a new type within the Greek political elite, not because of his personality or his manners but because of his wealth and his life-style? Or does he decide to dispense with that fact?

That question about Cleon exemplifies the concerns which underlie the much derided undergraduate rhetoric about "relevance."[49] Currently ancient history is a "growth area" in the curriculum, in Britain at any rate. Yet at precisely the same time—and often among the same students—fears, even guilt, about its relevance are increasingly audible. Is this a dilemma (or a weakness, depending upon the point of view) of the profession or of the consumer? And that leads me to ask once again, Who speaks for the ancient historians today? The criteria with which to judge are not easy to determine. By stressing the "histories of" and the learned journals, I have indicated where my answer lies. It is then not surprising that I see more retrogression than progress from Grote and Mommsen (the Mommsen of the *Römische Geschichte*). Each of their successive volumes was eagerly anticipated and discussed at length in the most important cultural organs of their day. Rightly so.[50]

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[1]Paul Veyne, *Comment on écrit l'histoire* (Paris: Editions du Seuil, 1971), p. 255. Although his subtitle is "Essai d'epistémologie," I am not here concerned with that aspect.

[2]Ibid., pp. 253 and 271, respectively.

[3]Ibid., p. 267.

[4]Throughout, unless otherwise indicated, my remarks refer only to ancient historians in the nonsocialist countries.

[5]Martin Nilsson, *Greek Popular Religion* (New York: Columbia University Press, 1940), pp. 4, 15, and 135, respectively. I have no reason to alter the criticisms I made in a review-article in *Studies in Philosophy and Social Science*, 9 (1941):502-510.

[6]I have switched from Greece to Rome because the only book that comes to mind to balance against Thucydides is the first volume of G. B. Grundy, *Thucydides and the History of His Age* (London: John Murray, 1911), a flawed and (wrongly) forgotten work.

[7]Quoted from A. Momigliano's introduction to an Italian translation of the *Kulturgeschichte*, reprinted in his *Secondo Contributo alla storia degli studi classici* (Rome: Edizioni di Storia e Letteratura, 1960), pp. 283-298, at p. 286 n. 4, where other dismissive judgments are also cited.

[8]Karl Christ, *Von Gibbon zu Rostovtzeff* (Darmstadt: Wissenschaftliche Buchgesellschaft, 1972), pp. 157-158.

[9]I have recently called attention to one or two similar wish-fulfilling judgments in the field of ancient art, in a review-article, "In lieblicher Bläue," *Arion*, n.s., 3 (1976):79-95.

[10]For a fine appreciation of the magnitude of Mommsen's achievement, see A. Heuss, *Theodor Mommsen und das 19. Jahrhundert* (Kiel: Hirt, 1956), esp. chs. 2-4.

[11]Quoted from Christ, *Von Gibbon zu Rostovtzeff*, p. 94 n. 10.

[12]A. Momigliano, *Studies in Historiography* (London: Weidenfeld & Nicholson, 1966), p. 239.

[13]Further documentation will be found, via the Index *s.v.* Burckhardt, in Albert Wucher, *Theodor Mommsen. Geschichtschreibung und Politik* (Göttingen: Messerschmidt, 1956).

[14]Ludwig Lange, *Kleine Schriften* (Göttingen, 1887), vol. 2, p. 155.

[15]The phrase appears in the sympathetic appreciation of vol. 2 of the *Staatsrecht* by Jacob Bernays, "Die Behandlung des römischen Staatsrechtes bis auf Theodor Mommsen," in the *Deutsche*

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Rundschau for 1875, reprinted in his *Gesammelte Abhandlungen*, ed. H. Usener (Berlin: Hertz, 1885), vol. 2, p. 267.

[16]Hartmut Galsterer, *Herrschaft und Verwaltung im republikanischen Italien. Münchener Beiträge zur Papyrusforschung und antiken Rechtsgeschichte*, no. 68 (1976):4.

[17]Christ, *Von Gibbon zu Rostovtzeff*, pp. 106-108. "Political Pedagogy" is the title of ch. 3 of Wucher, *Theodor Mommsen*, who provides rich documentation of the reception of the *Römische Geschichte*. The much-discussed and extraordinarily complex question of Mommsen's unwillingness or inability to continue the narrative beyond the death of Julius Caesar is irrelevant in the present context.

[18]An 8-volume boxed set (Munich: Deutscher Taschenbuch Verlag, 1976).

[19]Veyne, *Comment on écrit l'histoire*, p. 263.

[20]The quotations are taken from Felix Gilbert's introduction to *Historical Studies Today*, edited by himself and S. R. Graubard (New York: Norton, 1972), pp. xvii-xviii. The individual contributions to the volume appeared initially in *Daedalus* (Winter and Spring 1971).

[21]Quoted from Uvo Hölscher, *Die Chance des Unbehagens* (Göttingen, 1965), p. 24.

[22]Preface by Graubard in Gilbert and Graubard (eds.), *Historical Studies Today*, p. ix.

[23]It is uncommonly refreshing to read in Galsterer, *Herrschaft und Verwaltung im republikanischen Italien*, p. 4, the open assertion that his break from Mommsen's *Staatsrecht* rests on no literary or juristic sources other than those "which were already Mommsen's starting-point."

[24]J. B. Bury had gone further in his Cambridge Inaugural (1903); he spoke of Mommsen's main contribution "as a historian" (the *Geschichte* gave him status as a "man of letters"); see his *Selected Writings*, ed. H. Temperley (Cambridge: Cambridge University Press, 1930), p. 9.

[25]M. Mandelbaum, "A Note on History as Narrative," *History and Theory*, 6 (1967): 413-419, at p. 417. I have discussed this question in ch. 3, "Generalization in Ancient History," of my *The Use and Abuse of History* (New York: Viking, 1975).

[26]W. R. Connor, *The New Politicians of Fifth-Century Athens* (Princeton: Princeton University Press, 1971).

[27]See now H. Bellen, *Studien zur Sklavenflucht im römischen Kaiserreich* (Wiesbaden: Steiner, 1971), pp. 118-122.

[28]I shall quote from the English translation by Janet Sondheimer (London: Weidenfeld & Nicolson, 1967).

[29]Reprinted in Vogt's *Ancient Slavery and the Ideal of Man*, translated by T. Wiedemann (Oxford: Blackwell, 1974), which I reviewed in the *Times Literary Supplement* for November 14, 1975.

[30]David Brion Davies, "Slavery and the Post-World War II Historians." *Daedalus*, 103 (Spring 1974):1-16, at p. 11.

[31]Eduard Meyer, "Die Sklaverei im Altertum," a lecture delivered and published in Dresden in 1898, reprinted in his *Kleine Schriften*, vol. 1 (2nd ed., Halle: Niemeyer, 1924), pp. 169-212.

[32]Vogt, *Slavery and the Ideal of Man*, pp. 209 and 25, respectively.

[33]Ibid., p. 103.

[34]No one can seriously hold this view after W. L. Westermann's powerful concluding pages in his *The Slave Systems of Greek and Roman Antiquity* (Philadelphia: American Philosophical Society, 1955), a work Vogt knows well.

[35]Vogt, *Slavery and the Ideal of Man*, p. 4.

[36]J. Deininger, in *Historische Zeitschrift*, 222 (1976):359-374.

[37]I am grateful for the assistance of a research student, Richard P. Saller of Jesus College, Cambridge, in checking and collating the reviews of De Martino.

[38]J. P. V. D. Balsdon, in *Classical Review*, n.s., 14 (1964):194-196, at p. 196.

[39]I single out H. Lévy-Bruhl, in *Revue historique de droit français . . .*, 4th ser., 30 (1952):139-140, and 34 (1956): 280-281; F. De Visscher, in *Studia et documenta historiae et iuris*, 21 (1955):370-378; and G. Scherillo (cited in n. 40), from whom my final quotation is taken. Perhaps the most surprising reaction is that of the *Zeitschrift der Savigny-Stiftung für Rechtsgeschichte (Romanistische Abteilung)*, for more than a century the most authoritative organ in the field. Although the review section in each volume runs well over 100 pages, with quite lengthy reviews, De Martino's *Storia* has never been given a review to itself, but has been restricted to two generally welcoming comments by one of the editors, Wolfgang Kunkel, in the course of long surveys of the new literature on the Roman constitution: 72 (1953):297, and 77 (1960):370-375.

[40]G. Scherillo, in *Iura*, 6 (1955):335-349.

[41]M. A. R. Colledge, in *Classical Review*, n.s., 21 (1971):253-255.

[42]Mason Hammond, "The Antonine Monarchy: 1959-1971," in H. Temporini (ed.), *Aufstieg und Niedergang der römischen Welt*, vol. II 2 (Berlin and New York: De Gruyter, 1975), p. 338.

[43]Balsdon, in *Classical Review*.

[44]E. S. Staveley, in *Journal of Roman Studies*, 50 (1960):250-253. "Admit" appears four times in the review as a synonym for "accept my view." Lest the distinction between the Franco-Italian reception and the rest appear to be total, I quote one sentence from Frezza's review of the first

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volume, in *Studia et documenta historiae et iuris*, 18 (1952):279-291, at p. 280: "To find a justification for the author's polemical tone . . . one should think not just of an interest in comprehending the content of human institutions, but of an interest in comprehending them according to the doctrine of dialectical materialism."

[45]See De Martino's lengthy reply, "Di E. S. Staveley o dell' intolleranza nella critica storica," *Labeo*, 8 (1962):241-264.

[46]J. H. Hexter, *Reappraisals in History* (2nd ed., London: Longmans, 1967 impression), p. 118. He attributes the formulation to the late Edward Kirkland but gives no source.

[47]Franz Bömer, *Untersuchungen über die Religion der Sklaven in Griechenland und Rom* (Akad. der Wissenschaften u. der Literatur in Mainz, *Abhandlungen der geistes- u. sozialwissenschaftlichen Klasse*, 1957, no. 7; 1960, no. 1; 1961, no. 4; 1963, no. 10).

[48]R. W. Fogel and S. L. Engerman, *Time on the Cross* (2 vols., Boston and Toronto: Little, Brown, 1974).

[49]See further my chapter in J. H. Plumb (ed.), *Crisis in the Humanities* (Penguin, 1964), and ch. 12 in my *Use and Abuse of History*; Karl Christ, "Römische Geschichte in Unterricht," *Geschichte in Wissenschaft und Unterricht* (1972) Heft 5:277-290.

[50]As often in the past, John Dunn and C. R. Whittaker have made helpful suggestions.

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STEPHEN TOULMIN

From Form to Function: Philosophy and History of Science in the 1950s and Now

I

THE 1960S WERE A TIME of striking changes throughout the intellectual and artistic worlds. None of those who grew up and entered academic life during the preceding thirty years could live through that decade without feeling that the landmarks of his mental world were being eroded, shifted, or even swept away. My aims in this essay are (1) to chronicle the effect of these changes on the subjects I have been best acquainted with at first hand, viz., the philosophy of science and the history of science; and (2) to see whether such a chronicle can point us toward any more general or long-term conclusions about the sixties, or about the cultural transformations they brought with them.

As we finally left the fifties behind us, the old familiar "world of learning" seemed stable and unchanging. The territory of science, scholarship, and the arts was still divided up into so many separate and varied disciplines, media, and fields; and, within each of these areas, the specialized procedures and technical values of those disciplines preserved their full force and authority. To be a true professional, it was necessary to commit oneself to a specific field, and to develop "sound" habits of work. The goal was *excellence*, and professional "excellence" was perceived as attainable only within a field in which well-defined standards of achievement were acknowledged and applied.

As late as 10 or 15 years ago, indeed, the very notion of "interdisciplinary" studies—the desire to raise questions lying in the gray areas between established disciplines—was itself widely regarded with suspicion. There might be a place for some such questions in the context of liberal education, at any rate on the undergraduate level; but beyond that point they were likely only to distract us. It was our business as sound academics to do well those things we had the means of doing well. Good, solid, well-disciplined science or scholarship might be "hard" in the sense of being difficult and demanding, but it was also "hard" in the sense of yielding secure, trustworthy results. Through their very nature, by contrast, interdisciplinary studies were doubly "soft": they were a standing invitation for soft-centered investigators to pursue ill-defined inquiries, the outcomes of which were both unreliable in themselves and built on insecure foundations. Academic "soundness" required rigor of a kind that, in these gray interdisciplinary areas, was simply not there to be had.

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To take one step further: for the intellectual world of the 1950s, such terms as *rigor* and *technique* tended to mean, more often than not, formal rigor and formal technique. The focus of academic attention was on the general, the abstract, and the timeless. Physics, for instance, still aspired above all to be mathematical and universal: though the specific content of Einstein's generalized field theory might arouse his colleagues' skepticism, his underlying cosmological ambitions (inherited ultimately from Parmenides) were very generally shared by other physicists. As for mathematics, it was still focused largely on its own structure and foundations, and for those who had grown up under the shadow of a Hilbert or a Hardy the material existence of electronic computers had no more serious disciplinary significance than the material existence of hot dogs. Philosophy, for its part, still aspired to be logical: analytical philosophers, logical empiricists, and phenomenologists alike followed Frege in rejecting concessions to "historicism" or "psychologism"—like Kant, they saw attention to the empirical facts of "mere anthropology" as sapping the foundations of philosophy's rigorous deductions—while the suspension of interest in ideology had put political philosophy into temporary eclipse. Elsewhere, again, the goals of disciplinary development were widely perceived as yielding new styles of formal technique, with the help of which literary criticism (or whatever it might be) could acquire a professional rigor of its own, and so enter upon "the sure path of a science."

Similar abstract, technical preoccupations had been dominant for some time in many of the fine arts, too. In the 1950s such figures as Schoenberg and Mondrian kept their authority undiminished: Mahler and Klimt, on the other hand, were still regarded as odd and dated individuals, whose work had had no significant issue. By a masterly feat of abstraction, twentieth-century musical theorists and artists had apparently succeeded in detaching musical composition from orthodox classical tonality, and painting from the naturalistic representation of particular concrete objects; and, by so doing, they had put those arts on the road toward developing their own bodies of general theory and technique, uncluttered by all the varied and fluctuating demands of human perception and sensibility. For the time being, it seemed as unthinkable to reverse those aesthetic achievements as it would have been to abandon the intellectual goals of a Frege or a Hilbert, or to revive a serious philosophical concern with, say, the historicist preoccupations of a Fichte or a Hegel.

That is how matters stood—at any rate in the Anglo-American academic world—as we entered the 1960s. Yet now, only about fifteen years later, it takes a real effort of mind to put ourselves back into that frame of thought and perception. By 1976, the pursuit of abstract and universal ideas has become curiously out of fashion, as compared with the concrete analysis of particular historicocultural episodes and situations. Mere formalism no longer seems of deep interest, at least when divorced from considerations of function; and the minutiae of disciplinary expertise need to be explicated and justified, by being applied to specific instances and cases. Nowadays, we seek to develop not so much timeless theories about the general nature of "social groups" and "social action" as historical insights into the character and experience of this or that human group or collectivity; to grasp not so much the general statics of cultural equilibrium as the dynamics of particular cultural changes; to achieve not so

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much the formal rigor of axiomatic systems as the practical testability and computability of programs and algorithms.

Within the limits of our professional academic enterprises, we have in this way achieved some kind of "relevance" to the facts of human experience, and we derive a new kind of intellectual satisfaction from the "concreteness" of the resulting discoveries. That satisfaction no longer rests on any formal, Cartesian warranty of deductive necessity: rather it comes from a Viconian assurance that we have succeeded in coming to grips with human actualities. And, if the price of reestablishing this contact with actual experience includes breaking down the fences separating the established academic disciplines so that we can bring their varied techniques and theories to bear on some specific practical problems, so be it. These days, we are all prepared to be "interdisciplinary."

II

My own philosophical involvement with the natural sciences goes back to the 1930s, and my deeper motives for choosing to study mathematics and physics as an undergraduate were (as I later came to see) philosophical rather than technical. Yet there was little enough going on by way of philosophy of science in Britain in 1946, when I returned to Cambridge University to do postgraduate work in philosophy; and this was still the case when I took up a lectureship in philosophy of science at Oxford in 1949. My own supervisor of research, Richard Braithwaite, was developing his ideas about the formal character of scientific explanation, along lines hinted at earlier by the late Frank Ramsay. At the London School of Economics, Karl Popper had recently arrived from New Zealand and was beginning to build up a local following for his arguments about fallibilism, falsifiability, and the "logic" of scientific research. Elsewhere, one or two logicians and scientists were experimenting with the "axiomatic" method, which, for example, J. H. Woodger applied to analyzing the structure of biological theory. But the main tide of formalist philosophy of science, generated in Vienna in the years before Hitler, overleaped Britain and flowed directly to the United States.

So, when I began lecturing at Oxford during 1950, there was nothing one could call an orthodoxy in the philosophy of science in Britain. Moving from Cambridge to Oxford—from the minute Moral Sciences Faculty still marked by the impress of the later Wittgenstein, and surrounded by a largely scientific university, to a philosophical community of some 50 teachers and 1000 students, largely insulated from the real-life natural sciences—I accordingly chose to deal with the tasks most directly to hand. I sought, that is, to extend and reapply Wittgenstein's analysis of "language games" and "methods of representation" to the life and work of natural science: showing, as best I could, how the actual "language games" operative in that life are in fact played, and so indicating how—and why—the more formal "inductive logic" approach presupposed by English logicians since the time of J. S. Mill missed the serious philosophical points that arise in the course of scientific work. Scientific theories and scientific explanations (I argued) in practice employ "representations" of many different kinds, and they can be usefully analyzed as deductive or axiomatic systems only in special cases and on special conditions. Rather than

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allowing the axiomatic method of analysis any monopoly, what was most needed was a functional taxonomy of explanatory procedures and techniques, which would relate these procedures to the problematics of different kinds of scientific inquiry. That, at any rate, was the implication of my first, introductory book on the philosophy of science, which was published in 1953.[1]

Meanwhile, in the United States, the position of the subject was very different. There, the philosophy of science was heavily influenced by the influx of "logical empiricists" from Austria and Germany: notably, Carl Hempel and Herbert Feigl, Rudolph Carnap and Hans Reichenbach, John von Neumann and Philip Frank. These men had much admired Wittgenstein's early *Tractatus Logico-Philosophicus*, but they saw no interest in his later work. Instead, they developed a "positive" program and method for philosophy-in-general which wedded the logical techniques of Russell and Whitehead's *Principia Mathematica* to an empiricist epistemology borrowed from Ernst Mach; and they found in the formal rigor and empirical basis of natural science (the "deductive validity" of its inferences and the "verifiability" of its propositions) a touchstone for judging the intellectual adequacy of other enterprises, too. Or rather—as we might now say—they found this touchstone in the *supposed* formal rigor and empirical basis of natural science, *as they saw it*. But their actual perception of natural science was strongly refracted by two other extrascientific factors. The first of these was the fact that the philosophers concerned had come to the philosophy of science mainly from the philosophy of mathematics, or from formal logic. The second was the longer-term intellectual goal of the program: viz., that of building up a single, comprehensive axiom-system, having Russell's analysis of mathematics as its formal core, which should be capable in principle (with the help of enough supplementary "axioms") of representing the totality of our positive scientific knowledge. Hence was born the "Unified Science" movement of the 1930s.[2]

The German-speaking emigré philosophers of the logical-empiricist movement found natural allies among the more formally minded of the younger American pragmatists, such as Ernest Nagel. For both groups of men, all genuine problems in the philosophy of science were to be construed as problems about the "logical structure" of the sciences, rather than about the psychology of scientific discovery or about the historical evolution of scientific concepts. Indeed, the one point of most general agreement among them all was probably their acceptance of Hans Reichenbach's distinction between the "context of discovery" and the "context of justification." In their view, the empirical goings-on of the scientific life and the process of discovery—whether the psychological vicissitudes of individual scientists, or the collective histories of scientific groups—were matters for the behavioral and social sciences, and had nothing to do with the philosophy of science: that could begin only when questions of justification arose, that is, when scientific work provided material to formulate an explicit *argument*, whose validity, evidential force, and cogency could be exposed to logical scrutiny.

Euclid had set out the hypotheses and proofs of theoretical geometry in a rigorous, timeless system of deductions, which had little direct connection with the practical activities of ancient Greek "geometricians" (*sc.*, surveyors or landmeasurers), and in this way he had made plain the force and necessity of geometrical inferences. In the same way, twentieth-century philosophers

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should seek to formalize the logical structures of the natural sciences, so as to make their intellectual credentials equally plain, beginning with physics and working outward from there. The central preoccupations of the philosophy of science, as so conceived, were thus with the calculus of probabilities, with "confirmation theory," and with other technical procedures for analyzing the formal relations among the *propositions* of science.[3] The *concepts* of science, in terms of which those propositions were framed, were for the moment taken for granted. In the case of mathematical concepts, such as *number*, Frege had argued that the meanings of the terms concerned can be inferred from their context in the corresponding formal systems; and the logical empiricist program implicitly assumed that this argument could be extended to the theoretical concepts of the natural sciences, too.

If the positive program of this formalist philosophy of science was highly influential in the United States, its negative program was perhaps even more so. For about twenty years, the prohibition against confusing *formal or logical* issues (for which read "rational" issues) with *empirical* matters (for which read "non-rational," or even "irrational," matters) whether in the history, the psychology, or the sociology of science, was widely observed even among people who were otherwise quite far from the logical empiricists in their thinking. To be convicted of "the genetic fallacy"—as having muddled historical origins with logical justifications—was to risk being laughed out of court, and the same went for the parallel offenses of "psychologism" or "sociologism." And one significant measure of the distance we have moved in the last ten or fifteen years is the fact that, today, many philosophers of science can hear themselves charged with psychologism or the genetic fallacy without turning a hair.

We can sum up the dual aspects of the orthodox approach to philosophy of science in the United States of the 1950s in three statements. The central convictions were: (1) that careful scrutiny and analysis of the arguments which emerge within the scientific "context of justification" will reveal that properly conducted natural science does indeed have a canon, "method," or organon; (2) that the essential procedures of that method can be captured and expressed in formal algorithms, relating the empirical observations of science to the theoretical propositions in terms of which they are to be explained; and (3) that the "rationality" of the natural sciences lies in conforming to that set of formally valid procedures.

The positive goal of the resulting philosophy of science was to establish the content of this organon. Its negative goal was to protect us from the fallacies that might otherwise distract us from the properly rational structure of science: notably—to use Francis Bacon's term—from the "idols" of history, psychology, and sociology. And what united these two elements was a common central ambition: both to demonstrate the essential rationality of the "scientific method," and to give an analytical account—what Plato would have called a *logos*—of that "rationality" in algorithmic terms.

III

By the mid-fifties, it was becoming clear to some younger philosophers of science that certain crucial questions on the subject could not be tackled with any hope of success, unless they set aside all formal or "logical" issues, and paid

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attention instead to the processes of historical change out of which the basic concepts, theories, and methods of science have emerged, and to which they are continually subject. The rationality of science cannot depend solely on the formal validity of the inferences drawn within the scientific theories of any given time. (By itself, that can do little more than protect scientists from inconsistency and incoherence.) We can recognize the source of science's explanatory power only if we come to understand also what is involved in the processes of conceptual change: in particular, how the character of these processes can give authority to new concepts, new theories, even new methods of thought, inquiry, and argument. The current content of a science can be represented as a system of "propositions" linked by "formal inferences" only when we have at our disposal a common, agreed-upon vocabulary of terms and concepts to serve as the "subjects" and "predicates" of all those propositions; and, manifestly, all really profound changes in scientific thought and theory have brought with them correspondingly profound changes in the basic vocabulary of scientific terms and concepts. Once we turn to the really basic level of theoretical interpretation and conceptual variability, as a result, a formalist approach is *necessarily* insufficient, and the architectural metaphors of formal logic—"structure" and the rest—must be set aside in favor of some other analysis of scientific work.

Accordingly, I began to take a close first-hand interest in the history of science during an exchange visit to Melbourne University, Australia, in 1954–1955; and this remained the chief focus of my academic concerns for the next dozen years or so. Moving into this new discipline, I found a situation parallel to that in the philosophy of science. Professional historians of science shared a fairly well-defined positive conception about the scope of their common disciplinary tasks; but they shared an even clearer negative conception about the problems, questions, and issues that did not fall within that scope. Embarrassingly, I found that philosophical issues were included among the latter group of suspect topics. Approaching the history of science with philosophical questions in mind seemed as heretical to professional historians of science in the 1950s as approaching the philosophy of science with historical questions in mind did to most of their philosophical colleagues. (Just how far both groups of scholars shared a common intellectual attitude toward science was, at the time, not yet generally apparent.)

True: in 1955, the history of science was still a young subject, at any rate as a formally organized and established discipline. Apart from a single department at University College, London, British universities provided hardly any teaching positions in the subject. By and large, historians in Britain still perceived themselves as "humanists," in an exclusive sense of the term. They were prepared to write political, diplomatic, and even intellectual history; but they commonly thought of social history as a subject less for serious scholars than for children—*Daily Life in Ancient Greece*, etc.—while the history of science, like the history of ideas, was in Britain a "nonsubject." (When G. M. Trevelyan published his *English Social History* in the late forties, it was a nine days' wonder, and even he made only fleeting references to science and technology; as for Herbert Butterfield's *Origins of Modern Science*, that appeared a courageous and groundbreaking tour de force.) Meanwhile, in the United States, George Sarton

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had been plowing a lonely furrow at Harvard's Widener Library for about twenty-five years: professional scientists generally viewed the history of science as a frivolity, professional historians in most cases viewed it as an irrelevancy, and the few new posts in history of science coming available were justified less in scholarly terms than as contributing to undergraduate general education.

Once the history of science began to "take off" in the United States around 1950, a general agreement quickly developed among the scholars directly involved, about the proper topics of research within the field. In particular, they focused their discussion sharply on issues "internal to" the intellectual development of the various natural sciences. They investigated, for instance, the evidence and arguments by which Lavoisier was led to his theory of chemical "elements"; they looked to see who had published the first papers generalizing the concept of "energy"; they even began to study the intellectual context and origins of Einstein's special theory of relativity. In each case, they were at pains to investigate how precisely the crucial experiments had been devised, who exactly could claim priority for his formulations, with what justification certain lines of theoretical innovation had been followed up rather than others. In each case, that is to say, they made it their business to pursue, retrospectively, those same kinds of professional judgments of merit that are debated and agreed on today within the actual practice of current science.

By contrast, most historians of science took care to steer clear of questions about the social context, economic motivation, and political priorities that had helped to shape the different natural sciences at this time and place. All such questions about the "external context" of science were dismissed by the profession as intellectually soft, theoretically peripheral, and too easily misconceived. It is not that anybody ever set out to demonstrate that these "externalist" issues were downright incapable of being framed in precise, acceptable terms. In fact, a very few professional sociologists of science were granted a special dispensation to deal with such problems: notably, Robert Merton, whose writings had undeniable merits even from the strict standpoint of professional historians.[4] For themselves, however, historians of science chose to draw a sharp line between the *content* of science and its *context*, and stayed securely within the narrower circle of "internalist" issues. So, young graduate students whose imaginations began wandering further afield quickly became sensitized to those subtle academic pressures summed up in the lift of an eyebrow and the interrogative murmur of the single word, "unsound?" (As late as 1964, for instance, when Everett Mendelsohn of Harvard published the first of his papers on the social context of science, he recalls feeling "very much alone.")[5]

How are we to account for these self-imposed limitations? All those questions about the external relations of science that have been attracting the attention of professional historians of science since the later 1960s could perfectly well have been posed at any time during the previous fifty years. That is, they were questions that had earlier been, not so much unavailable, as positively avoided. The subsequent shift of focus in the history of science, and the resulting changes in the professional "self-definition" of the field, thus sprang less from genuine innovations and discoveries than from the dissipation of earlier professional fears and disciplinary prejudices.

Many factors were no doubt involved in shaping the history of science in

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this way, and we must not oversimplify our picture of the situation. There are those who would say that, given the political atmosphere in America during the 1950s, it was important for historians of science to avoid being suspected of "communistic" tendencies; and it is true that, among the few historians of science who always ignored the taboo on externalist problems, a fair number—for example, J. D. Bernal and Benjamin Farrington—were self-professed Marxists.[6] To make matters worse, a paper delivered by the Soviet historian Boris Hessen in the 1930s to a conference in London (perhaps as a tease?) had purported to reveal the true roots of Netwon's *Principia* in the social and economic life of seventeenth-century Britain; and so made all too evident the intellectual limitations of a purely externalist approach. (There had been nothing like it since revisionist Biblical scholars had suggested that, after all, the *Song of Songs* was really *meant* to be read as an erotic poem!) For several decades, indeed, Hessen's paper served as an object lesson to scare younger scholars in the history of science away from social questions; and Hessen himself meanwhile came to a bad end, during the Stalinist purges of the late thirties.[7]

Still, there was certainly more to these internalist prejudices than the lingering constraints of McCarthyism. In the first place, the general avoidance of externalist issues was not confined to the United States: the same concentration on internal conceptual problems was found among most European scholars also, whether or not their countries had been caught up in Red Scares. In the second place, it was not merely socioeconomic issues of traditional leftish kinds that were avoided: all questions that went far outside the content of the actual scientific argument were equally suspect. If orthodox Newton scholars were scornful of Hessen's paper, they were downright scandalized by Frank Manuel's *Portrait of Isaac Newton*, published as late as 1968, just as the change of emphasis in the subject was getting underway.[8] Manuel's *Portrait* used psychobiographical methods to trace some of the Master's intellectual ambitions back to the effects of infantile desertion. (Newton's father died before he was born, and his mother put him out to foster parents at the age of 18 months.) It had been bad enough to see the Theory of Universal Gravitation treated as a byproduct of seventeenth-century mercantilist capitalism, but it was even less acceptable to have its splendid intellectual architectonic presented as the outcome of Freudian neurosis. Either way, it seemed, the magnitude of Newton's intellectual achievement—of the *Principia* as an examplar of rational inquiry into Nature—was denied, and credit for its appearance given rather to nonrational—if not outright irrational—factors or considerations.

At this point, the basic assumptions of historians of science in the fifties came close to those of their empiricist colleagues in philosophy of science. Sarton himself, for instance, clearly held a somewhat naive empiricist view of the tasks and methods of natural science. He would not allow that philosophical thought had ever contributed to the advance of science, and pointed to Plato's supposed scientific muddleheadedness as an object lesson in confusion. In his view, the scientist must approach the observed facts of Nature in a simple, unprejudiced frame of mind, and those facts would then "speak for themselves." State your question; do an experiment; get your answer—that was the straightforward message, and the history of scientific thought was a record of good, rational empiricists seeking to follow this rule, in the face of interference from

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bad, irrational theologians and metaphysicians.[9] Once again, the fact that we cannot even state a question without taking for granted the applicability of the concepts in terms of which it is framed did not apparently impress Sarton. In his eyes, as in those of the logical empiricists, honest observation and valid deduction between them should do the job; and, whether or not the canons of scientific inference were strictly formalizable, the "rationality" of science certainly consisted in recognizing and respecting those canons.

So, while conscientiously refraining from any interdisciplinary collaboration—which would have been against both their principles—historians and philosophers of science in the United States of the 1950s went parallel ways, under the influence of their common commitment to an empiricist conception of "scientific method." If the philosopher's task was to establish the formal organon of science, the task for the historian was to produce "rational reconstructions" of the scientific achievements of the past. He should set himself, that is, to trace out and put in order the intellectual steps by which our predecessors were able to build up, course by painful course, the House of Learning we ourselves inhabit. This meant accepting the classic texts of natural science as intellectual "masterpieces," or "holy objects," and considering only the rational procedures and arguments by which their creators had been able to formulate and justify them. Given this focus, we can well understand why the orthodox methodology in the history of science during the late forties and fifties paid exclusive attention to the experiments internal to the sciences; and also why external factors of all or any kinds—psychological and philosophical, as much as social or economic—were seen as distracting attention from this central enterprise.

IV

At some time around 1965, the self-imposed limitations within which both historians and philosophers of science had been working ever since the end of World War II were quietly removed. The change took place without any public fanfare, by tacit agreement. Whereas Everett Mendelsohn had felt very much on his own when writing about historical changes in the social context of science in 1964, five years later the barriers between history and sociology were clearly down, and there was even a vogue among younger historians of science for exploring issues falling across the boundary between the two subjects.[10] In the philosophy of science, similarly, the chief focus of attention shifted quite rapidly from the "structure" of scientific theories to the "dynamics" of scientific change, and the citing of quite detailed documentation from the historical record of scientific change—even if only by way of exemplifying philosophical theses—soon became quite acceptable, if not commonplace.[11] In neither field did anybody actually stop to explain just why this change was taking place: why problems that had been taboo five years earlier were now okay, why questions that it had been disreputable to push until recently were now respectable. But, somehow or other, the message quickly got around that the older academic constraints were no longer being enforced. The electric chair had apparently been disconnected, and everyone hurried to take turns at sitting in it.

In the philosophical area, we can recognize in retrospect three sets of issues

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that played a central part in this transition. (1) The need to confront the problems of scientific change would probably have been enough, by itself, to impose some fresh attention to the facts of history; and, indeed, the objections to "psychologism" and "sociologism" have been slower to disappear. In the hands of philosophers like Norwood Russell Hanson, at any rate, historical case studies became powerful philosophical weapons.[12] It had been all very well for the logical empiricists to develop a purely formal and abstract organon of "inductive logic" for the natural sciences, so long as nobody asked how that set of formal canons was to be given specific applications to real-life scientific practice. But it now began to be called in question, not merely whether the formal algorithms of "confirmation theory" and the rest were capable of being applied in practice as they stood—few of the logicians concerned had ever made that claim—but also whether there was any possibility, even in principle, of reframing them in terms relevant to the conceptual issues that actually face working scientists in the context of their professional activities. That now became the crucial question for the philosophy of science, and it was a question that could be settled only by taking the record of history seriously. By the early 1970s, as a result, public debates in the philosophy of science were repeatedly turning into analyses of key episodes in the historical evolution of science; and the scholarly burden of proof had shifted so far that these historical issues had to be faced and dealt with, even by those who still wished—like Imre Lakatos, and other associates of Karl Popper—to keep alive the ideal of a timeless and universal "rational organon" for all genuine natural science.[13]

In addition, however, two other lines of argument reinforced those to do with scientific change. To begin with, (2) it began to be apparent that the orthodox empiricist approach to the philosophy of science had rested on some very substantial and questionable assumptions: e.g., about the possibility of identifying basic "facts of observation" quite independently of all theoretical considerations, and so formulating "hard" empirical propositions (what the Vienna Circle philosophers had called *Protokolsaetze*) to serve as the epistemological foundations of science. Between 1965 and 1975, several younger philosophers—notably Dudley Shapere and his associates at Chicago and Urbana—set about taking these assumptions apart, and they soon began to appear very flimsy.[14] The inductive logic approach might be one legitimate approach to the philosophy of science among others, but it was certainly not the only one; and all attempts to make it compulsory by freeing it from "metaphysics" turned out, on closer scrutiny, to rest on their own dubious metaphysical presuppositions. Meanwhile, (3) the positivists' earlier preoccupation with "prediction," which they had regarded as the key evidence of scientific "knowledge," was being supplemented by a concern with "explanation," regarded as the core of scientific "understanding." For certainly, if we look at the actual tasks of working science, we shall find that not all prediction is scientific—or horserace tipsters would be scientists—and not all science is predictive, or evolutionary biologists would not be scientists. (Faced with this distinction between "predicting" and "explaining" natural phenomena, the hardier positivists simply retorted that, if evolution theory was not predictive, then it could not really be a "science"; but this seemed a desperate way of dealing with a genuine difficulty.[15])

Worse: this new attention to the character of "explanation" in working

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science began to undercut the formalist approach at its very foundations. In the empiricists' book, the prototypical example of appealing to a theoretical *explanans* to account for an observational *explanandum* had always consisted in producing a universal "theoretical" proposition (e.g., "All ravens are black") as logical support for a particular "observational" proposition (e.g., "This raven is black"). But now the question arose, whether this kind of model, after all, had any real relevance to the "explanations" given in working science. Certainly, if the blackness of any one particular bird gave rise to scientific puzzlement in practice—if it was perceived as "demanding explanation"—no professional scientist would rest content with being told that the same question arose about all members of the species: "Oh, *all* ravens are black, are they? Now, that really *does* need explaining!" And it would commonly be agreed that a genuine explanation had been produced, only if a further account were given, which went way beyond all simple entailments and other formal relations, and related the bird's color to quite other kinds of facts: for example, facts about the biochemical morphogenesis of the pigments in its plumage, or about the life environments and selective pressures to which evolutionary predecessors of the modern species had been subjected. To count as a "scientific explanation," that is to say, the account of a raven's blackness would have to shift to quite another conceptual level, involving a kind of theoretical reinterpretation whose merits cannot be captured in a merely formal algorithm.

After 1965, historians of science also expanded their range of concerns in several different directions. Most notably, they embarked on sociologically oriented investigations: looking at the institutions of professional science, and at the influence of institutional factors on the character of scientific work. This was, of course, an area in which Robert Merton and his colleagues, such as Joseph Ben-David, had already broken the ground[16]; and it soon became hard to tell at first glance whether one should classify certain writers as historical sociologists or as sociologically minded historians.[17] It is probably no accident that this new interest became active just at a time when there began to be widespread disquiet about the current institutions of American science: here as elsewhere, we might perhaps learn something about our present discontents by looking back at the problems of other times and peoples. In this respect, the questions facing historians of science have become only the more urgent during the subsequent ten years. Current controversies about the political accountability of scientists, especially those arising out of the proposal to "regulate" research on recombinant DNA, have opened up a whole new range of issues about the "political theory" of natural science, which can be dealt with satisfactorily—like all questions about political theory—only if tackled in a way that is historically sensitive and informed. As a result, professional historians like Mendelsohn find themselves being called in as "consultants" by both sides in the recombinant DNA dispute.

The doors out of history into the psychology of science have been slower to open: in this direction, Manuel's Newton portrait has had no convincing successors. Still, the earlier armlock exerted on historians of science by naive conceptions about "scientific method" has been noticeably easing off. Within the field of Newtonian studies, for instance, it is no longer a matter of such deep embarrassment that the great Sir Isaac should have been interested in alchemy,

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and we are at last beginning to be able to see the real significance of alchemical investigations within the seventeenth-century intellectual context.[18] Even George Sarton's strong prejudice against historians of science consorting with philosophers is, by now, largely a thing of the past. We can date the formal rapprochement to the International History of Science Congress at Ithaca, New York, in 1967. There the question, whether historians of science have anything to learn from philosophers of science or vice versa, was debated solemnly and officially; but, since then, the collaboration has become a routine matter, calling for only the minimum of apologetics. In this direction, the work of Thomas Kuhn has had a healthy long-term effect: both by example, because he prompted scholars to confront issues in the history of science which he himself stated in highly philosophical language; and also by reaction, because historians and philosophers have had to join forces in order to criticize his initial formulations effectively.

It is perhaps worth trying to state concisely the central implications of this broadening of scope for both the history and the philosophy of science. On the one hand, most philosophers of science would by now agree that the whole story about the organon of science cannot be told in the formal language of inductive logic alone. Even those who remain committed to the program of developing a formal theory of scientific confirmation or corroboration, if only as an ideal, would acknowledge that its relevance and application to the actual natural sciences can be grasped only with the help of historical examples. The maxims of scientific methodology cannot be presented simply as abstract principles, in the realm (so to say) of scientific jurisprudence: in addition, we must assemble a body of "case law" to flesh them out and give them content. Meanwhile, those who would restrict the scope of inductive logic even further—such as myself—would argue that scientific methodology itself needs to be developed, rather, on a "common law" model, by the collection and restatement of precedents: the logician cannot simply bring "the tablets of the law" down to the scientist from his axiomatic Sinai.

On the other hand, the change in perspective since the mid-sixties has put historians of science on notice about the danger of a new kind of anachronism: so long as they went along with Sarton's views about the supreme role of "experiment" in science, and about the irrelevance (not to say, the corrupting effect) of philosophy, they could cheerfully take it for granted that scientists of all kinds—whatever their topics of research, whatever their countries, and whatever their dates of birth and death—might be studied and judged with an eye to the same universal recipes for "good, rational scientific investigation." Now, the existence of such general, timeless recipes can no longer be taken for granted. We must look and see afresh, in every case, what considerations in fact weighed with (for example) Newton the alchemist/cosmologist/chronologist, in developing his ideas about force, gravitational attraction, "active powers," and all the rest. To bring fixed and preformed standards of judgment to such historical investigations is to duck the crucial issues of relevance and irrelevance. (In this respect, Karl Popper's ambition, to formulate a definitive "demarcation criterion" for identifying all genuine *science*, needs to be discussed collaboratively between philosophers and historians of science: it is basic not just to the

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claims of any formal "logic of scientific research," but also to the historiography of scientific theories and conceptual evolution.)

V

At this point, I want to broaden the canvas a little, and begin working my way back to my initial topic: viz., the general significance of the 1960s. In both the philosophy of science and its history (as we have seen) the last ten or fifteen years have seen a real shift in our attitudes toward the idea of "rationality," and this shift has deeply affected the ways in which scholars pose—and conceive of—their topics of research and debate. Just how are we to describe that shift? It is not possible to do so unambiguously, for its nature and implications are themselves one of the central points under discussion. As to that, current views range along a spectrum. At one extreme, there are those who would speak of a shift away from a previously exaggerated concern with the rationality of science. At the other, there are those who would speak rather of a reorientation within or an expansion of our ideas about rationality. Let me briefly spell out these alternatives.

As the best exemplar of the first tendency, let me take the work of Paul Feyerabend. Having earlier on been strongly influenced by Popper, with his devotion to the *Logik der Forschung* as a timeless embodiment of the ideals of rationality, Feyerabend has more recently reacted against Popper; and he has carried his attack not merely against the claims for an organon or method in science, but also against the idea that progress in scientific thought can—or should—be the product of rational inquiry.[19] At times, Feyerabend's attacks on the role of reason in science remind one of David Hume's pregnant epigram to the effect that "The Reason is, and ought to be, the Slave of the Passions." But, where Hume writes with delicate irony, Feyerabend is in deadpan earnest. For him, reason and imagination preserve their traditional philosophical antagonism, so that any reinstatement of the imagination in scientific thinking necessarily carries with it a corresponding rejection of (or limitation on) the reason. We might be tempted to ask, in reply, "Cannot the reason be used imaginatively? Must the reason, in the act of guiding the passions, thereupon cease to be 'rational'?" But Feyerabend is clear in his reply. In being Against Method, he is also determined to be Against Rationality. Having given the dog a bad name, he is determined to beat it.

In this, Paul Feyerabend has found natural allies. Those who followed the development of the counterculture during the 1960s will recognize the rejection of "scientific rationality" as one of the chief themes of its romantic ideology. In the canonical writings of Theodore Roszak, for instance, the attacks of Goethe, Schiller, and Blake on the repressive influence of Newtonian science are repeated as applying to contemporary science in its entirety; and the claims of the imagination are restated in direct contradistinction to those of "rationality."[20] The social and political uses of this antiscientific and antirationalist position are familiar enough; but it is worth remarking that, during his time as Professor at the Free University of West Berlin, Feyerabend became something of an idol to the radical activists. His arguments seemingly justified a new and

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creative liberation of thought and action from the restraints of the rational and conventional, not merely in science itself, but in a wider sphere. (If even Science is hampered by an excessive preoccupation with "rationality," then is not the same surely true a fortiori of Ethics and Politics?)

At its heart, however, this view was not necessarily a radical or irrationalist one. Indeed, one of those who has been most strongly drawn toward it has been Kuhn, who seems in other respects a man of more conservative temperament. As he himself put the point, the science of any particular epoch is dominated by a "ruling paradigm": this overarching intellectual frame determines not just the respectable content of (say) good Newtonian science, but also its respectable methods of argument. The Great Books of natural science serve (that is to say) not just as exemplary products of rational inquiry into Nature, but also as Books of the Law. They decide what shall, for the time being, constitute "rationality" in science. They set the rational norms to be followed by the next few generations of students working within what Kuhn himself calls Normal Science. For his own part, Kuhn has strongly resisted all those who criticize the relativistic elements in his position as driving him inevitably into an irrationalism or antirationalism similar to Feyerabend's[21]; and he was apparently much embarrassed when the radical students at Columbia showed signs of adopting him as a Prophet—"What we need is a new Paradigm for Society!"—as the Berlin radicals had done with Feyerabend.

Yet it is hard to see how Kuhn can ultimately hold his critics at bay. If we are to reject the positivists' ideal of a single formal and universal organon for science, binding equally in all cultures and all epochs, it is not enough simply to replace it by the notion that each culture and epoch is entitled to have its own distinct repertory of formal procedures for science. Accordingly, the thrust of the other main tendency in the current history and philosophy of science is to argue that we must replace all narrowly formal conceptions and analyses of rationality by broader functional ones. It is not the universality of the positivists' claims that we need to combat, as Kuhn attempts to do when he rejects the absolutism of traditional "scientific method" in favor of half-a-dozen relativized ("Aristotelian," "Newtonian," or "Einsteinian") methods of doing science. Rather, it is their formalistic character: their concentration on the logical character of the relations between the propositions of science, instead of on the explanatory fertility of its concepts. From this alternative point of view, the current shift in our attitudes toward rationality need involve no rejection of "rational" inquiry or action in practice; rather, it involves a reanalysis of the nature and content of "rationality" on the theoretical plane. Ever since Plato and his associates idealized formal geometry as the exemplar of *episteme*, philosophers had been tempted to follow their lead, and to make formal deductive structure—preferably, axiomatic structure—the test of a truly "rational" science. Yet surely the time may now have come at last to recognize that formal structures, as such, cannot be entirely self-justifying in science. They, too, have to be justified, in turn, by their explanatory fruits.

So conceived, the joint task for historians and philosophers of science becomes, not so much to establish and exemplify the formal and universal Ten Commandments of the scientific method, as to map out the historical development of its jurisprudence: showing how, as we move from one age or branch

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of science to another, the progressive changes in the standards of "good argument" and "rational inquiry" have been functional responses to the changing problematics of the different sciences in different milieux. What is characteristically rational about scientific inquiry, from this point of view, will then no longer be the conformity of scientific argument to permanent formal canons, but rather the manner in which scientists have changed their procedures and patterns of argument, in any particular area of theory, investigation, or debate, so as to adapt them better to the demands of its changing problems. And, because the intellectual problems facing natural scientists in the twentieth century impose different demands on scientific thought today from those confronting Plato or Galileo or Lavoisier (say), it should be no surprise that the intellectual procedures developed to deal with them are equally different from those that carried authority in fourth-century Athens or seventeenth-century Italy. On the contrary, any uncritical commitment of scientists to a single, general set of formal canons should raise the question, whether they were not in danger of allowing a taste for formal elegance to blind them to the functional demands of their investigations.

To say this is not to reject the idea that certain formalizable techniques and standards may well have their place in the procedures of the natural sciences: at least, in a given branch of science at one particular time. Certainly, for example, there is room to develop general formal treatments of Mill's Methods for the discovery of causes, or of the statistical significance tests for judging experimental designs. But the practical scope and application of such methods has to be considered subsequently on a case-by-case basis. (Mill's Methods are fine in agricultural testing, for instance, but they have very little relevance to quantum physics.) Rather than attempting to give a complete analysis of "scientific rationality" in either formal or functional terms alone, therefore, we should perhaps recognize that both aspects have legitimate claims on our attention. There are some respects in which the intellectual claims of science are bound up with the existence of clear and well-formalized intellectual techniques for doing "scientific" research; but it is equally important that, from time to time, these techniques should themselves be called in question, and modified to meet the functional demands of new cases and problems. To some extent, of course, both types of inquiries go on within natural science all the time. The critically minded scientist is always prepared to reappraise his own techniques in the course of applying them. (This is the methodological point lying behind Popper's overly propositional thesis about the eternal falsifiability of scientific truths.) Still: it is worth asking whether, in actual fact, there is not a tendency within any science for one or another aspect to be emphasized at any one time, and so for the methodological pendulum to swing between periods of comparative technical stability and periods of rapid technical change.

To go further: it is worth asking, also, whether there is not a similar alternation to be observed between periods in which the focus of scientific inquiry is on the development and refinement of general intellectual techniques for their own sake, in abstraction from any concern with specific concrete instances or applications; and periods in which the focus shifts from such general techniques to particular kinds of concrete problems and problem-situations. Just as the fundamental concern within any intellectual discipline

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may be either with exploring the ramifications and uses of a given set of techniques and procedures, or else with seeking to find ways of improving them, so too the fundamental concern across the boundaries separating intellectual disciplines may be either to foster the autonomy of the various fields in refining their own proper general procedures, or else to find ways of drawing on their pooled resources to attack particular concrete problems and applications. Either concern can, of course, be pursued in a "rational" manner; but it will be a different aspect, or mode, of rationality in either case.

With this possibility in mind, we can return to the points I made at the outset. The change most characteristic of the academic world during the 1960s and early 1970s, I suggest, was a change from an orientation on techniques to an orientation on applications. The academic world I myself grew up in was organized around autonomous, self-defining disciplines and departments, each of which claimed the right to settle its own agenda by reference to the special intellectual techniques and procedures that were its inheritance and stock in trade. Such an organization left no room for "interdisciplinary" work, which would have to focus less on the improvement of these disciplinary skills than on their possible utility in concrete situations, falling across the boundaries separating different intellectual domains. The slogan of this period was "excellence," but that term was used as something of a codeword. In the 1950s, academic excellence was equated with technical refinement, and the insistence on pursuing excellence at all costs too often meant, "Leave us alone to do our own academic thing. Take away your 'concrete interdisciplinary problems.' We don't want to hear about them!"

The current shift in academic preoccupations, by contrast, is bringing about changes not just in the orientation of research, but in institutional organization also. (Prototypically, one might consider the discussions at MIT, over the question whether the institute's work might not be structured in part around, e.g., problems of transportation, communication, etc., rather than entirely around, e.g., the techniques of electrical engineering, civil engineering, etc.) So understood, the alternative codeword with which the critics of the sixties countered the advocates of academic "excellence"—viz., "relevance"—can at any rate be given some content. For, indeed, the academic preoccupations of the seventies are not merely interdisciplinary in a way they could not be twenty years ago: they are also focused on problems and issues that are relevant to specific human applications. The general academic skills built up with such pain and care over the previous forty or fifty years are now being put to work, in dealing with problems that require us to mobilize the resources of several disciplines to serve specific human needs.

VI

To put the changes in academic life during the sixties into a broader context, I am suggesting that they represent one aspect of a wider shift in attitudes and preoccupations: a change that has extended not only across the whole world of learning and the arts, but into the practical realms of politics and social life also. Between 1910 and 1960, the cutting edge of originality in the arts and sciences was kept sharp by an emphasis on technical, and specifically on formal methods

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and ideals—by taking mathematical logic as a foundation for philosophical analysis, by basing theoretical physics on the formal algorithms of the tensor calculus and quantum mechanics, by developing the Cartesian, geometrical style in architecture, nonrepresentational modes of painting, and twelve-tone techniques in musical composition—in general, by turning away from the rich, concrete particularity of history and nature into an alternative, timeless, and theoretical world of abstractions, set over against the confusions of historical actuality.[22]

During those years, correspondingly, the academic and artistic professions moved into a new phase of specialization. Each "discipline" or "profession" was characterized by, and organized as the custodian of, its own corpus of formal techniques, into which newcomers had to be initiated and accredited, as apprentices. So, there was a general tendency for each of the professions to pull away from its boundaries with others, and to concentrate on its own central, essential concerns. Where the culture of fin de siècle Europe had welcomed versatility—Arnold Schoenberg was a talented writer and painter was well as a musician, and in the years around 1900 the flow of ideas among artists, scholars, and scientists in different fields was lively and serious—during the period between the two World Wars, culture was dominated by "experts": tightly organized groups of artistic or academic technicians, each of them in jealous control of its own body of formal craft mysteries.

If those of use who grew up into that cultural world came to perceive the structure of the academic professions as excessively narrow and constraining, that fact should not need explaining. And the abrupt lurch by which this whole cultural picture was shaken loose, and pieced together again in a fresh pattern, should be no more of a surprise. For quite suddenly, during the 1960s, artistic and academic preoccupations whose authority and domination had been unquestioned for about forty years lost their power to command loyalty and attention. Professional philosophers of science began seriously studying the historical development of science; theoretical physicists acquired a new pragmatism; painters turned back from their extreme avoidance of representation to an equally exaggerated revival of realism; Mahler gained ground at the expense of Schoenberg and Webern; while even the purest of pure mathematicians found it acceptable to dabble with computers. All across the spectrum, scholars and scientists returned to confront the historical, temporal world in all its concrete detail, and the formal abstractions of the previous generation were dismissed—sometimes too harshly—as "irrelevant," if not as "elitist." After forty years of concentration on a timeless abstract world of Being, the resulting revival of interest in the concrete temporal processes of Becoming has certainly favored a shift from narrowly "discipline-oriented" research to wider and more "problem-oriented" issues; has resulted in a general lowering of the intellectual barriers separating different academic disciplines; and has made possible, even desirable, a greater involvement of the lay public in the affairs of the learned professions.

In this last respect, the changes in the academic world are in phase with, and in tune with, current changes of emphasis in the broader social or political world. In his interesting studies of the politics of science, Don K. Price has ranged problems along what he calls "the spectrum from Truth to Power," and his way of viewing the interrelations between "truthseeking" academics and

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"powerwielding" politicians or administrators has thrown a lot of light on the political vicissitudes of American science.[23] But there is a corresponding range of different problems to be recognized, within both the academic and the practical spheres. During "discipline-oriented" phases, the accepted foci of academic study are defined in relation to an abstract, specialized view of Truth, which has no direct relationship to, or concern with, the power to do Good; whereas, during "problem-oriented" phases, these foci are reorganized and redefined in relation to a more concrete, generalized point of view, according to which the capacity for Good is a necessary element in the Truth itself. In the social or political sphere, likewise, we can recognize an alternation between antiideological phases of "consensus politics," in which the focus is on Means, or on Efficiency—on "how to do it"—and ideological phases of "confrontation politics," in which the focus is on Ends, or on Justice—on "what to do, and for whom." And it is probably no accident that the sixties and early seventies have seen, not merely a shift in intellectual preoccupations from the True to the Good, but a contemporaneous shift in political preoccupations from Means to Ends, from Efficiency to Justice.

Yet what is the longer term significance of these changes, and what is the prognosis for their continuation? Are we to see them as authentic novelties—as permanent, possibly even irreversible, changes in our interests and preoccupations? I suspect not. If we view the sixties against the whole historical background of Western life and culture, the changes we have been living through in the academy seem to me to have been simply one more swing of a familiar pendulum, by which thought and art have moved, every sixty or hundred and twenty years, between formal and functional, classic and romantic, timeless and temporal, Platonist and Aristotelian extremes. For myself, therefore, I doubt whether the current era of interdisciplinary, problem-oriented, historically and culturally informed research will last much longer than its predecessors. Instead, I would be inclined to bet that, by the early twenty-first century, a new Hilbert and Frege, a new Schoenberg and Mondrian, will be leading us back into abstract formalism, technical concentration, and professional specialization. Not that the precise areas of specialization will be the same next time around as they were in the years between 1920 and 1960. After a generation of concentration on interdisciplinary, concrete issues, new constellations of technical problems will be abstracted out to serve as the foci of new disciplines; and these will then need about thirty years to develop their own specialized theoretical ideas and techniques to the required pitch of excellence, before they in turn are reapplied by a more practically minded generation to the concrete human problems of their time.

The image that comes to my mind is that of a kind of folkdance, which alternates periods of marching and periods of weaving. For a time, the different academic professions march forward separately but in parallel, each in its own special way; then, for a time, they join hands and work together on the general problems arising in the areas where their techniques overlap; only to break away once more into separate lines, and march along in fresh directions until they are ready to join hands again. This has happened before and, if it happens again, fine: this alternation is probably the only way in which we can prevent our scholarly and scientific concerns from either hardening into a permanent

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professional scholasticism, or softening into a morass of well-meaning imprecision. That is to say, it is probably the only way in which, in the long run, the academic enterprise can strike a proper balance between the legitimate claims of Truth and Efficiency on the one hand, Goodness and Justice on the other.

REFERENCES

[1]Stephen Toulmin, *The Philosophy of Science: An Introduction* (London, 1953): cf. also my later short book, *Foresight and Understanding* (Bloomington, Ind., 1961). In working on these books, I learned greatly from the discussion of "methods of representation" in W. H. Watson's *On Understanding Physics* (Cambridge, England, 1938), which had been the only previous extended discussion of the philosophy of science by a student of Wittgenstein's later period.

[2]For a fuller treatment of this topic, see my papers on "Scientific Strategies and Historical Change," in R. J. Seeger and R. S. Cohen (eds.), *Boston Studies in Philosophy of Science*, vol. 11 (Dordrecht, 1973), pp. 401-414, and "Rationality and Scientific Discovery," in K. F. Schaffner and R. S. Cohen (eds.), *Boston Studies in Philosophy of Science*, vol. 20 (Dordrecht, 1974), pp. 387-406.

[3]The classic texts in this tradition are C. G. Hempel, *Aspects of Scientific Explanation* (New York, 1965); Rudolf Carnap, *Logical Foundations of Probability* (Chicago, 1950); and Ernest Nagel, *The Structure of Science* (New York, 1961): all these books present arguments and positions developed and discussed over the previous thirty years.

[4]Merton's monograph, "Science, Technology and Society in 17th-Century England," published in Sarton's periodical *Osiris* in 1938, had established his historical qualifications. But the question arises whether he too might not have come under professional suspicion from historians as possibly "unsound," if he had not been primarily a sociologist.

[5]Personal conversation, July 1976.

[6]See, for instance, B. Farrington, *Science in Antiquity* (London, 1936), and J. D. Bernal, *Science in History* (London and New York, 1965). Fear of being labeled as Marxists is the prime reason suggested by Everett Mendelsohn for the avoidance of externalist issues by American historians of science during the 1950s.

[7]Boris Hessen, "The Social and Economic Roots of Newton's *Principia*," in *Science at the Crossroads* (London, 1933): reprinted and discussed several times in recent years, e.g., by G. Bassalla in *The Rise of Modern Science: External or Internal Factors?* (Lexington, Mass., 1968).

[8]F. E. Manuel, *A Portrait of Isaac Newton* (Cambridge, Mass., 1968).

[9]Sarton's antiphilosophical stance is clear in all his writing about the methods to be employed in history of science research, from his *Introduction to the History of Science* (Baltimore, 1927) right up to his *Guide to the History of Science* (Waltham, Mass., 1952).

[10]Significantly, a number of new journals dealing with the historical sociology of science and related topics began publication during the last ten years, and during the same period some significant new professional societies were established: most recently, the international group calling itself the Society for Social Studies of Science. Meanwhile, the longer established history of science quarterlies, such as *Isis*, have also begun to take on a new and more socially oriented tone. In addition, many American colleges and universities have begun to use "Science and Society" as an area of concentration for undergraduate courses. (All this is quite apart from the development of "science policy" studies in universities and other research centers—a development that had rather different origins, in such practically oriented agencies as the OECD, Paris, and the National Academy of Sciences, Washington.)

[11]For two other, fuller accounts of this transition in the philosophy of science, written from rather different philosophical standpoints, see (1) the introductory essay by Fred Suppe to the collection on *The Structure of Scientific Theories*, F. Suppe (ed.) (Urbana, Ill. 1974) and (2) the paper by T. Kisiel (with G. Johnson), "New Philosophies of Science in the U.S.A.," *Zeitschrift fur allgemeine Wissenschaftstheorie*, V, 1 (1974): 138-191. Both accounts, incidentally, cite my own 1953 and 1961 books (see note 1 above) as "the beginning of the new movement" away from the formal ahistorical procedures of inductive logic to a more pragmatic and historically oriented style of analysis. This is personally gratifying to me, but also puzzling, for the direct influence of those books seems to me to have been comparatively slight, and this attribution accordingly raises more questions about the world of professional philosophy than it does about my own personal achievements.

Granted: when I wrote these two books in question, especially the earlier one, I certainly felt as much "out in the cold" as Everett Mendelsohn did in the early 1960s, when he began publishing papers about the social context of the history of science. Yet the reasons I wrote as I did had little to do with any missionary desire to convert my professional colleagues to my own position, or to turn the philosophy of science in a new direction. At the time (I suppose) I imagined that I need only set the facts out clearly enough, and my professional colleagues would thereupon change their minds:

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what is referred to as "the courage of one's convictions" is often simple naivete. At any rate, it never occurred to me to try and organize any "party" or pressure group, or to attempt any collective assault on the academic strongholds in which the inductive logicians and logical empiricists were so securely entrenched. Indeed, by the time that Shapere and others (see note 14) were taking up similar lines of argument, and undertaking the hard grind of presenting them within the forums of the profession in terms that would carry conviction, my own mind was off somewhere else: moving by way of the history of science into the borderland between epistemology and the sociology of knowledge. (See, for instance, my *Human Understanding*, Princeton, N.J., 1972).

So, if the work I did at Oxford in the early 1950s now appears in retrospect to have been the start of a movement, I can claim no credit for that fact. On the contrary: if I had known then how far it might be possible to reorient the wider professional discussion in the philosophy of science, it would have been remiss of me to do as little as I did in that direction. And, as things worked out, the new style of philosophizing about science came into favor during the late 1960s, not because of anything I had done, but for quite other reasons. So, the best that could justly be said of my work during the 1950s is that I had been (so to say) the Mendel of the subject, whose methods and results had to be rediscovered and developed afresh by others at a later stage, when the collective professional debate had moved onto a fresh terrain, where they could at last be digested, applied, and put to fruitful use.

[12]See, for instance, N. R. Hanson, *Patterns of Discovery* (Cambridge, England, 1958), and *The Concept of the Positron* (Cambridge, England, 1963).

[13]One notable feature of Lakatos's later papers just is the way in which they became progressively more concerned with historical analysis, even in the service of what is—ostensibly, at least—an antihistoricist thesis. Indeed, his last major paper (written jointly with Elie Zahar for a symposium at UCLA in November, 1973) was a historicocritical reappraisal of Copernicus's achievement in the light of his own theory of "progressive" and "degenerating" research programs.

[14]See, for instance, Shapere's paper, "Notes towards a Post-Positivistic Interpretation of Science," in P. Achinstein and S. F. Barker (eds.), *The Legacy of Logical Positivism* (Baltimore, 1969).

[15]See for instance the debate between Carl Hempel and Michael Scriven, as documented in Hempel, *Aspects of Scientific Explanation*, with crossfire from my own *Foresight and Understanding*: in the interests of preserving his "covering law" model of explanation, Hempel is prepared to dismiss all claims that evolution theory can "explain" anything.

[16]See, for instance, Joseph Ben-David, *The Scientist's Role in Society* (Englewood Cliffs, N.J., 1971). Ben-David has written most illuminatingly, for instance, about the development of physiology during the nineteenth century, drawing attention to the differences between the directions of development of the subject in France (where it was cultivated alongside the hospitals) and in Germany (where it was primarily a university subject).

[17]How productive has this new tolerance among historians of science, toward psychological, sociological, and other "nonrational" factors, yet been? Has it put us in a position to understand the complex interactions between the inner (rational) structures or activities of the scientific profession and the larger (causal) world of social, cultural, and personal influences within which they develop? To say that, even now, would be going too far: all that has changed, so far, is that externalist issues are being discussed alongside, and in addition to, the older internalist issues, instead of being swept aside as irrelevant. Though the resulting inquiries are giving us some excellent raw material to work with, most historians of science (it must be said) are as far as ever from understanding how these two kinds of processes and interactions bear upon, and affect, one another.

[18]See, for instance, Betty Jo Teeter Dobbs, *The Foundation of Newton's Alchemy* (Cambridge, England, 1976).

[19]Feyerabend's major exposition of his position, *Against Method* (London, 1975), is significantly subtitled "Outline of an anarchistic theory of knowledge."

[20]T. Roszak, *The Making of a Counter-Culture* (New York, 1969) and *Where the Wasteland Ends* (New York, 1972): for a fuller discussion of the romantic affiliations of the "antiscience" movement of the 1960s, see my own paper, "The Historical Background to the Antiscience Movement," in the CIBA symposium, *Civilization and Science* (Amsterdam, 1972), pp. 23-32.

[21]See the reply to his critics printed as the final essay in *Criticism and the Growth of Knowledge*, I. Lakatos and A. Musgrave (eds.) (Cambridge, England, 1970).

[22]See also my article, "On Rediscovering History," in *Encounter* for January 1971; and chs. 8 and 9 of A. Janik and S. Toulmin, *Wittgenstein's Vienna* (New York, 1973).

[23]See especially Don K. Price, *The Scientific Estate* (Cambridge, Mass., 1965).

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LEWIS THOMAS

Biomedical Science and Human Health: The Long-Range Prospect

IT IS CUSTOMARY to place the date for the beginnings of modern medicine somewhere in the mid-1930s, with the entry of the sulfonamides and penicillin into the pharmacopoeia, and it is usual to ascribe to these events the force of a revolution in medical practice. This is what things seemed like at the time. Medicine was upheaved, revolutionized indeed. Therapy had been discovered for great numbers of patients whose illnesses had previously been untreatable. Cures were now available. As we saw it then, it seemed a totally new world. Doctors could now *cure* disease, and this was astonishing, most of all to the doctors themselves.

It was, no doubt about it, a major occurrence in medicine, and a triumph for biological science applied to medicine—but perhaps not a revolution after all, looking back from this distance. For the real revolution in medicine, which set the stage for antibiotics and whatever else we have in the way of effective therapy today, had already occurred 100 years before penicillin. It did not begin with the introduction of science into medicine. That came years later. Like a good many revolutions, this one began with the destruction of dogma. It was discovered, sometime in the 1830s, that the greater part of medicine was nonsense.

The history of medicine has never been a particularly attractive subject in medical education, and one reason for this is that it is so unrelievedly deplorable a story. For century after century, all the way into the remote millennia of its origins, medicine got along by sheer guesswork and the crudest sort of empiricism. It is hard to conceive of a less scientific enterprise among human endeavors. Virtually anything that could be thought up for the treatment of disease was tried out at one time or another, and, once tried, lasted decades or even centuries before being given up. It was, in retrospect, the most frivolous and irresponsible kind of human experimentation, based on nothing but trial and error and usually resulting in precisely that sequence. Bleeding, purging, cupping, the administration of infusions of every known plant, solutions of every known metal, every conceivable diet including total fasting, most of these based on the weirdest imaginings about the cause of disease, concocted out of nothing but thin air—this was the heritage of medicine up until a little over a century ago. It is astounding that the profession survived so long, and got away with so much with so little outcry. Almost everyone seems to have been taken in. Evidently one had to be a born skeptic, like Montaigne, to see through the old

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nonsense; but even Montaigne, who wrote scathingly about the illnesses caused by doctoring centuries before Ivan Illich, had little effect. Most people were convinced of the magical powers of medicine and put up with it.

Then, sometime in the early nineteenth century, it was realized by a few of the leading figures in medicine that almost all of the complicated treatments then available for disease did not really work, and the suggestion was made by several courageous physicians, here and abroad, that most of them actually did more harm than good. Simultaneously, the surprising discovery was made that certain diseases were self-limited, got better by themselves, possessed, so to speak, a "natural history." It is hard for us now to imagine the magnitude of this discovery and its effect on the practice of medicine. The long habit of medicine, extending back into the distant past, had been to treat everything with something, and it was taken for granted that every disease demanded treatment and might in fact end fatally if not treated. In a sober essay written on this topic in 1876, Professor Edward H. Clarke of Harvard reviewed what he regarded as the major scientific accomplishment of medicine in the preceding fifty years, which consisted of studies proving that patients with typhoid and typhus fever could recover all by themselves, without medical intervention, and often did better for being untreated than when they received the bizarre herbs, heavy metals, and fomentations that were popular at that time. Delirium tremens, a disorder long believed to be fatal in all cases unless subjected to constant and aggressive medical intervention, was observed to subside by itself more readily in patients left untreated, with a substantially improved rate of survival.

Gradually, over the succeeding decades, the traditional therapeutic ritual of medicine was given up, and what came to be called the "art of medicine" emerged to take its place. In retrospect, this art was really the beginning of the science of medicine. It was based on meticulous, objective, even cool observations of sick people. From this endeavor we learned the details of the natural history of illness, so that, for example, it came to be understood that typhoid and typhus were really two entirely separate, unrelated disorders, with quite different causes. Accurate diagnosis became the central purpose and justification for medicine, and as the methods for diagnosis improved, accurate prognosis also became possible, so that patients and their families could be told not only the name of the illness but also, with some reliability, how it was most likely to turn out. By the time this century had begun, these were becoming generally accepted as the principal responsibilities of the physician. In addition, a new kind of much less ambitious and flamboyant therapy began to emerge, termed *supportive treatment*, and consisting in large part of plain common sense: good nursing care, appropriate bed rest, a sensible diet, avoidance of traditional nostrums and patent medicine, and a measured degree of trust that nature, in taking its course, would very often bring things to a satisfactory conclusion.

The doctor became a considerably more useful and respected professional. For all his limitations, and despite his inability to do much in the way of preventing or terminating illness, he could be depended on to explain things, to relieve anxieties, and to be on hand. He was trusted as an advisor and guide in difficult times, including the time of dying.

Meanwhile, starting in the last decade of the nineteenth century, the basic science needed for a future science of medicine got under way. The role of

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bacteria and viruses in illness was discerned, and research on the details of this connection began in earnest. The major pathogenic organisms, most notably the tubercle bacillus and the syphilis spirochete, were recognized for what they were and did. By the late 1930s this research had already paid off; the techniques of active and passive immunization had been worked out for diphtheria, tetanus, lobar pneumonia, and a few other bacterial infections; the taxonomy of infectious disease had become an orderly discipline; and the time was ready for sulfanilamide, penicillin, streptomycin, and all the rest. But it needs emphasizing that it took about fifty years of concentrated effort in basic research to reach this level; if this research had not been done we could not have guessed that streptococci and pneumococci exist, and the search for antibiotics would have made no sense at all. Without the long, painstaking research on the tubercle bacillus, we would still be thinking that tuberculosis was due to night air, and we would still be trying to cure it by sunlight.

At that time, after almost a century of modified skepticism about therapy amounting finally to near nihilism, we abruptly entered a new era in which, almost overnight, it became possible with antibiotics to cure outright some of the most common and lethal illnesses of human beings—lobar pneumonia, meningitis, typhoid, typhus, tuberculosis, septicemias of various types. Only the virus diseases lay beyond reach, and even some of these were shortly to come under control—as in poliomyelitis and measles—by new techniques for making vaccines.

These events were simply overwhelming when they occurred. I was a medical student at the time of sulfanilamide and penicillin, and I remember the earliest reaction of flat disbelief concerning such things. We had given up on therapy, a century earlier. With a few exceptions which we regarded as anomalies, such as vitamin B for pellagra, liver extract for pernicious anemia, and insulin for diabetes, we were educated to be skeptical about the treatment of disease. Miliary tuberculosis and subacute bacterial endocarditis were fatal in 100 percent of cases, and we were convinced that the course of master diseases like these could never by changed, not in our lifetime or in any other.

Overnight, we became optimists, enthusiasts. The realization that disease could be turned around by treatment, provided that one knew enough about the underlying mechanism, was a totally new idea just forty years ago.

Most people have forgotten about that time, or are too young to remember it, and tend now to take such things for granted. They were born knowing about antibiotics, or the drugs simply fell by luck into their laps. We need reminding, now more than ever, that the capacity of medicine to deal with infectious disease was not a lucky fluke, nor was it something that happened simply as the result of the passage of time. It was the direct outcome of many years of hard work, done by imaginative and skilled scientists, none of whom had the faintest idea that penicillin and streptomycin lay somewhere in the decades ahead. It was basic science of a very high order, storing up a great mass of interesting knowledge for its own sake, creating, so to speak, a bank of information, ready for drawing on when the time for intelligent use arrived.

For example, it took a great deal of time, and work, before it could be understood that there were such things as hemolytic streptococci, that there were more than forty different serological types of the principal streptococcal species

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responsible for human disease, and that some of these were responsible for rheumatic fever and valvular heart disease. The bacteriology and immunology had to be done first, over decades, and by the early 1930s the work had progressed just far enough so that the connection between streptococcal infection and rheumatic fever could be perceived.

Not until this information was at hand did it become a certainty that rheumatic fever could be prevented, and with it a large amount of the chief heart disease affecting young people, if only a way could be found to prevent streptococcal infection. Similarly, the identification of the role of pneumococci in lobar pneumonia, of brucellae in undulant fever, typhoid bacilli in typhoid fever, the meningococcus in epidemic meningitis, required the sorting out and analysis of what seemed at the time an immensely complicated body of information. Most of the labor in infectious disease laboratories went into work of this kind in the first third of this century. When it was finished, the scene was ready for antibiotics.

What was not realized then—and is not fully realized even now—was how difficult it would be to accomplish the same end for the other diseases of man. We still have heart disease, cancer, stroke, schizophrenia, arthritis, kidney failure, cirrhosis, and the degenerative diseases associated with aging. All told there is a list of around twenty-five major afflictions of man in this country, and a still more formidable list of parasitic, viral, and nutritional diseases in the less developed countries of the world, which make up the unfinished agenda of modern biomedical science.

How does one make plans for science policy with such a list? The quick and easy way is to conclude that these diseases, not yet mastered, are simply beyond our grasp. The thing to do is to settle down with today's versions of science and technology, and make sure that our health care system is equipped to do the best it can in an imperfect world. The trouble with this approach is that we cannot afford it. The costs are already too high, and they escalate higher each year. Moreover, the measures available are simply not good enough. We cannot go on indefinitely trying to cope with heart disease by open-heart surgery, carried out at formidable expense after the disease has run its destructive course. Nor can we postpone such issues by oversimplifying the problems, which is what we do, in my opinion, by attributing so much of today's chronic and disabling disease to the environment, or to wrong ways of living. The plain fact of the matter is that we do not know enough about the facts of the matter, and we should be more open about our ignorance.

At the same time—and this will have a paradoxical sound—there has never been a period in medicine when the future has looked so bright. There is within medicine, somewhere beneath the pessimism and discouragement resulting from the disarray of the health care system and its stupendous cost, an undercurrent of almost outrageous optimism about what may lie ahead for the treatment of human disease if we can only keep learning. The scientists who do research on the cardiovascular system are entirely confident that they will soon be working close to the center of things, and they no longer regard the mechanisms of heart disease as impenetrable mysteries. The cancer scientists, for all their public disagreements about how best to organize their research, are in possession of insights into the intimate functioning of normal and neoplastic

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cells that were unimaginable a few years back. The eukaryotic cell—the cell with a true nucleus—has itself become a laboratory instrument almost as neat and handy as the bacterial cell became in the early 1950s, ready now to be used for elucidating the mechanisms by which genes are switched on or off as developing cells differentiate or, as in the case of cancer cells, de-differentiate. The ways in which carcinogenic substances, or viruses, or other factors still unrecognized intervene in the regulation of cell behavior represent problems still unsolved, but the problems themselves now appear to be approachable; with what has been learned in the past decade, they can now be worked on.

The neurobiologists can do all sorts of things in their investigation, and the brain is an organ different from what it seemed twenty-five years ago. Far from being an intricate but ultimately simplifiable mass of electronic circuitry governed by wiring diagrams, it now has the aspect of a fundamentally endocrine tissue, in which the essential reactions, the internal traffic of nerve impulses, are determined by biochemical activators and their suppressors. The technologies available for quantitative study of individual nerve cells are powerful and precise, and the work is now turning toward the functioning of collections of cells, the centers for visual and auditory perception and the like, because work at this level can now be done. It is difficult to think of problems that cannot be studied, now or ever. The matter of consciousness is argued over, naturally, as a candidate for perpetual unapproachability, but this has more the sound of a philosophical discussion. Nobody has the feeling any longer, as we used to believe, that we can never find out how the brain works.

The immunologists, the molecular biochemists, and the new generation of investigators obsessed with the structure and function of cell membranes have all discovered that they are really working together, along with the geneticists, on a common set of problems: how do cells and tissues become labeled for what they are, what are the forces that govern the orderly development and differentiation of tissues and organs, and how are errors in the process controlled?

There has never been a time like it, and I find it difficult to imagine that this tremendous surge of new information will terminate with nothing more than an understanding of how normal cells and tissues, and organisms, function. I regard it as a certainty that there will be uncovered, at the same time, detailed information concerning the mechanisms of disease.

The record of the past half-century has established, I think, two general principles about human disease. First, it is necessary to know a great deal about underlying mechanisms before one can really act effectively; one had to know that the pneumococcus causes lobar pneumonia before one could begin thinking about antibiotics. One did not have to know all the details, not even how the pneumococcus does its damage to the lungs; but one had to know that it was there, and in charge.

Second, for every disease there is a single key mechanism that dominates all others. If one can find it, and then think one's way around it, one can control the disorder. This generalization is harder to prove, and arguable—it is more like a strong hunch than a scientific assertion—but I believe that the record thus far tends to support it. The most complicated, multicell, multitissue, and multiorgan diseases I know of are tertiary syphilis, chronic tuberculosis, and pernicious anemia. In each, there are at least five major organs and tissues involved,

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and each appears to be affected by a variety of environmental influences. Before they came under scientific appraisal each was thought to be what we now call a "multifactorial" disease, far too complex to allow for any single causative mechanism. And yet, when all the necessary facts were in, it was clear that by simply switching off one thing—the spirochete, the tubercle bacillus, or a single vitamin deficiency—the whole array of disordered and seemingly unrelated pathologic mechanisms could be switched off, at once.

I believe that a prospect something like this is the likelihood for the future of medicine. I have no doubt that there will turn out to be dozens of separate influences that can launch cancer, including all sorts of environmental carcinogens and very likely many sorts of virus, but I think there will turn out to be a single switch at the center of things, there for the finding. I think that schizophrenia will turn out to be a neurochemical disorder, with some central, single chemical event gone wrong. I think there is a single causative agent responsible for rheumatoid arthritis, which has not yet been found. I think that the central vascular abnormalities that launch coronary occlusion and stroke have not yet been glimpsed, but they are there, waiting to be switched on or off.

In short, I believe that the major diseases of human beings have become approachable biological puzzles, ultimately solvable. It follows from this that it is now possible to begin thinking about a human society relatively free of disease. This would surely have been an unthinkable notion a half century ago, and oddly enough it has a rather apocalyptic sound today. What will we do about dying, and about all that population, if such things were to come about? What can we die of, if not disease?

My response is that it would not make all that much difference. We would still age away and wear out, on about the same schedule as today, with the terminal event being more like the sudden disintegration and collapse all at once of Oliver Wendell Holmes' well-known one-hoss shay. The main effect—almost pure benefit, it seems to me—would be that we would not be beset and riddled by disease in the last decades of life, as most of us are today. We could become a healthy species, not all that different from the healthy stocks of domestic plants and animals that we already take for granted. Strokes, and senile dementia, and cancer, and arthritis are not natural aspects of the human condition, and we ought to rid ourselves of such impediments as quickly as we can.

There is another argument against this view of the future which needs comment. It is said that we are fundamentally fallible as organisms, prone to failure, and if we succeed in getting rid of one set of ailments there will always be other new diseases, now waiting out in the forest, ready to take their places. I do not know why this is said, for I can see no evidence that such a thing has ever happened. To be sure, we have a higher incidence of chronic illness among older people than we had in the early years of this century, but that is because more of us have survived to become older people. No new disease, so far as I know, has come in to take the place of diphtheria, or smallpox, or whooping cough, or poliomyelitis. Nature being inventive, we will probably always have the odd new illness turning up, but not in order to fill out some ordained, predestined quota of human maladies.

Indeed, the official public health tables of morbidity and mortality seem to be telling us this sort of thing already, even though, in all our anxiety, we seem

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unwilling to accept the news. We have already become in the Western world, on the record, the healthiest society in the history of humankind. Compared with a century ago, when every family was obliged to count on losing members throughout the early years of life, we are in a new world. A death in a young family has become a rare and dreadful catastrophe, no longer a commonplace event. Our estimated life expectancy, collectively, is longer this year than ever before in history. Part of this general and gradual improvement in health and survival is thanks to sanitary engineering, better housing, and, probably, more affluence, but a substantial part is also attributable, in recent years, to biomedical science. We have not done badly at all, and having begun so well I see no reason why we should not do even better in the future.

My argument about how to do this will come as no surprise. I say that we must continue doing biomedical research, on about the same scale and scope as in the past twenty years, with expansion and growth of the enterprise being dependent on where new leads seem to be taking us. It is an expensive undertaking, but still it is less than 3 percent of the total annual cost of today's health industry, which at last count was over $120 billion, and it is nothing like as expensive as trying to live with the halfway technologies we are obliged to depend on in medicine today; if we try to stay with these for the rest of the century the costs will go through the ionosphere.

But I should like to insert a qualification in this argument, which may be somewhat more of a surprise, coming from a doctor. I believe that the major research effort, and far and away the greatest investment for the future, must be in the broad area of basic biological science. Here and there, to be sure, there will be opportunities for productive applied science, comparable, say, to the making of polio vaccine or the devising of multidrug therapy for childhood leukemia, but these opportunities will not come often, nor can they be forced into existence before their time. The great need now, for the medicine of the future, is for more information at the most fundamental levels of the living process. We are nowhere near ready for large-scale programs of applied science in medicine, for we do not yet know enough.

Good applied science in medicine, as in physics, requires a high degree of certainty about the basic facts at hand, and especially about their meaning, and we have not yet reached this point for most of medicine. Nor can we predict at this stage, with much confidence, which particular items of new information, from which fields, are the likeliest to be relevant to particular disease problems. In this circumstance there has to be a certain amount of guessing, even gambling; and my own view is that the highest yield for the future will come from whatever fields are generating the most interesting, exciting, and surprising sorts of information—most of all, surprising.

It seems to me that the safest and most prudent of bets to lay money on is surprise. There is a very high probability that whatever astonishes us in biology today will turn out to be usable, and useful, tomorrow. This, I think, is the established record of science itself, over the past two hundred years, and we ought to have more confidence in the process. It worked this way for the beginnings of chemistry; we obtained electricity in this manner; using surprise as a guide we progressed from Newtonian physics to electromagnetism, to quantum mechanics and contemporary geophysics and cosmology. In biology, evolution

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and genetics were the earliest big astonishments, but what has been going on in the past quarter century is simply flabbergasting. For medicine, the greatest surprises lie still ahead of us, but they are there, waiting to be discovered or stumbled over, sooner or later.

I am arguing this way from the most practical, down-to-earth, pragmatic point of view. This kind of science is most likely, in the real world, to lead to significant improvements in human health, and at low cost. This is a point worth further emphasis, by the way: When medicine has really succeeded brilliantly in technology, as in immunization, for example, or antibiotics, or nutrition, or endocrine replacement therapy, so that the therapeutic measures can be directed straight at the underlying disease mechanism and are decisively effective, the cost is likely to be very low indeed. It is when our technologies have to be applied halfway along against the progress of disease, or must be brought in after the fact to shore up the loss of destroyed tissue, that health care becomes enormously expensive. The deeper our understanding of a disease mechanism, the greater are our chances of devising direct and decisive measures to prevent disease, or to turn it around before it is too late.

So much for the practical side of the argument. We need much more basic science for the future of human health, and I will leave the matter there.

But I have one last thing to say about biological science. Even if I should be wrong about some of these predictions, and it turns out that we can blunder our way into treating or preventing one disease or another without understanding the process (which I will not believe until it happens), and if we continue to invest in biological science anyway, we cannot lose. The Congress, in its wisdom, can't lose. The public can't lose.

Here is what I have in mind.

These ought to be the best of times for the human mind, but is not so. All sorts of things seem to be turning out wrong, and the century seems to be slipping through our fingers here at the end, with almost all promises unfilled. I cannot begin to guess at all the causes of our cultural sadness, not even the most important ones, but I can think of one thing that is wrong with us and eats away at us: we do not know enough about ourselves. We are ignorant about how we work, about where we fit in, and most of all about the enormous, imponderable system of life in which we are embedded as working parts. We do not really understand nature, at all. Not to downgrade us; we have come a long way indeed, just to have learned enough to become conscious of our ignorance. It is not so bad a thing to be totally ignorant; the hard thing is to be part way along toward real knowledge, far enough to be aware of being ignorant. It is embarrassing and depressing, and it is one of our troubles today.

It is a new experience for all of us. Only two centuries ago we could explain everything about everything, out of pure reason, and now most of that elaborate and harmonious structure has come apart before our eyes. We are *dumb*.

This is, in a certain sense, a health problem after all. For as long as we are bewildered by the mystery of ourselves, and confused by the strangeness of our uncomfortable connection to all the rest of life, and dumbfounded by the inscrutability of our own minds, we cannot be said to be healthy animals in today's world.

We need to know more. To come to realize this is what this seemingly

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inconclusive century has been all about. We have discovered how to ask important questions, and now we really do need, as an urgent matter, for the sake of our civilization, to obtain some answers. We now know that we cannot do this any longer by searching our minds, for there is not enough there to search, nor can we find the truth by guessing at it or by making up stories for ourselves. We cannot stop where we are, stuck with today's level of understanding, nor can we go back. I do not see that we have a real choice in this, for I can see only the one way ahead. We need science, more and better science, not for its technology, not for leisure, not even for health or longevity, but for the hope of wisdom which our kind of culture must acquire for its survival.

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Notes on Contributors

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